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CONTAGIOUS DISEASES

OF

DOMESTICATED ANIMALS.

INVESTIGATIONS

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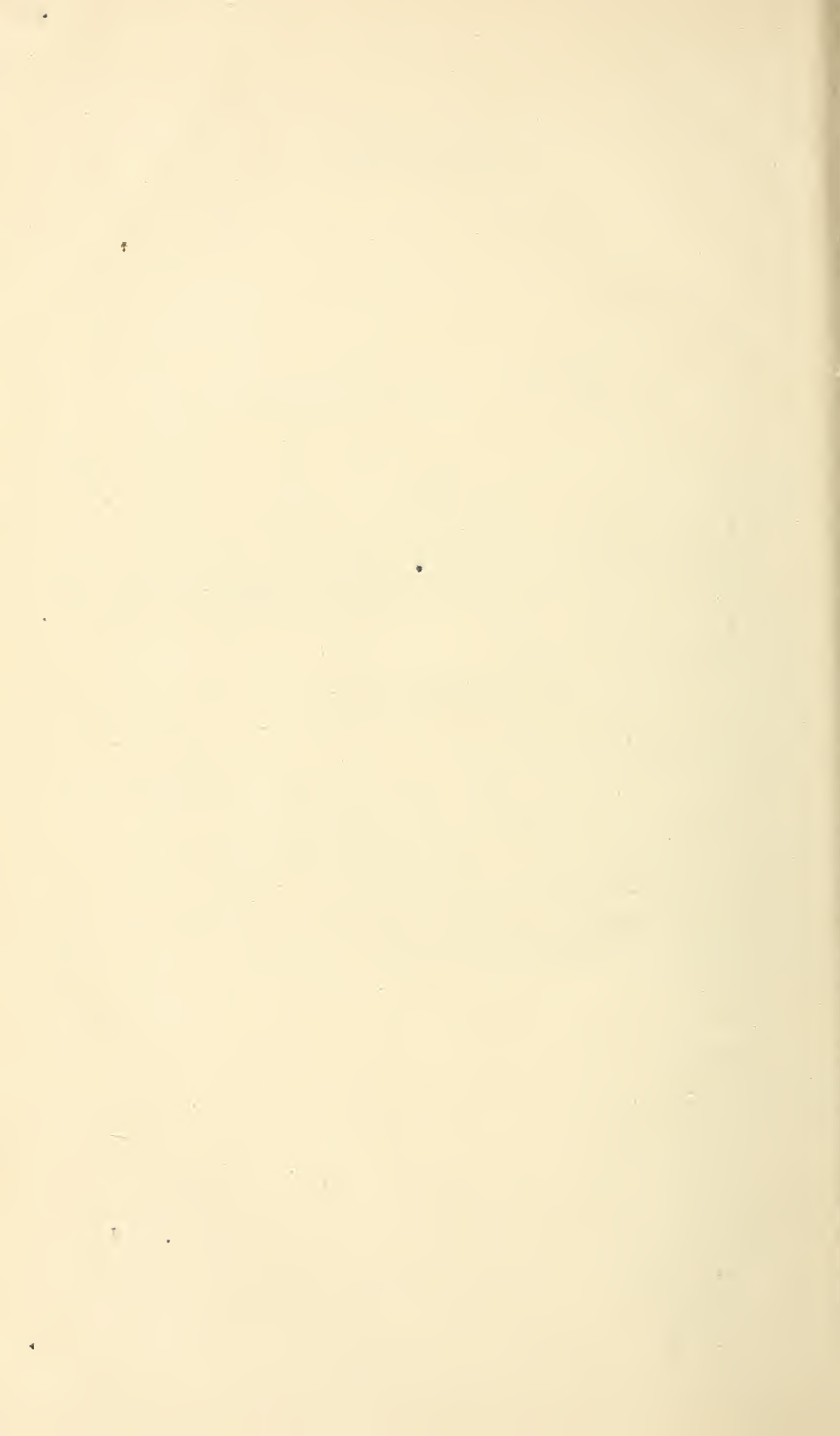
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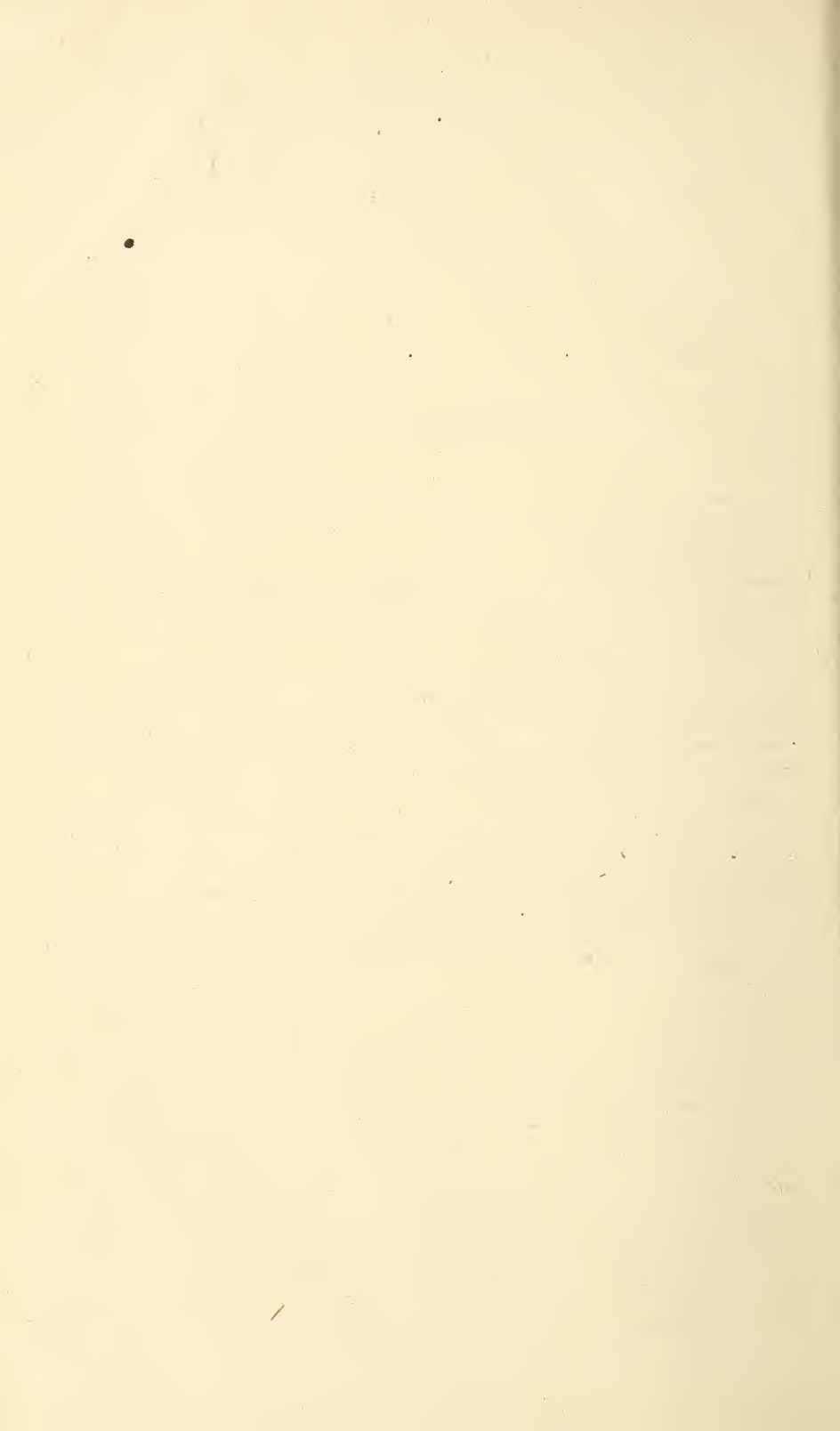
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INTRODUCTORY.

Dr. Salmon has devoted much time to the collection of facts bearing upon the present distribution in the State of Virginia of the disease generally known as Texas fever. This disease, which he proposes to call the Southern Cattle Fever, because of the wide area outside of Texas which is permanently infected with it, is shown to have invaded Virginia within a comparatively recent period and to have advanced over so much territory as to seriously affect the interests of that State.

It has long been known that cattle taken from any of the Northern States to some parts of the Southern States contract a disease which is very fatal to all but young animals, and it has generally been supposed that this was due to the increased temperature to which these imported cattle were necessarily subjected. Dr. Salmon shows that this is really the Texas or Southern fever of cattle, and that animals moved but a few miles, if they are brought from just beyond the border line of the infected district, are as subject to it and are affected with just as fatal results as those from the Northern States. He also furnishes facts which demonstrate that the native cattle from parts of Virginia where imported animals are known to die, if taken beyond these districts, distribute the same fatal infection as has long been known to be carried by those from Texas and the Gulf coast.

These facts, it is maintained, are exactly the same as those from which the conclusion was reached that Texas and the Gulf coast were infected with a fatal communicable fever; and that, consequently, there is no more reason to doubt the permanent infection of a part of Virginia than there is to doubt the similar infection of those regions in which this disease has been universally admitted to exist. While this conclusion is one of supreme importance to the cattle-raisers of Virginia, it must also have a very great interest for those engaged in this industry in all parts of our country. Indeed, the extensions of the railroad system in every direction, and the greatly increased facilities now offered for the shipment of cattle, make it a very common occurrence for animals of the improved breeds to be bought at the North and carried to the South, while the shipment of the fine animals that are now being bred in the South to various parts of the North is no longer a novelty. In addition to this there is the trade in stock and beef cattle, which is growing to enormous proportions. The danger connected with this trade, when it is not regulated by a knowledge of the district infected with Texas fever, and of the precautions necessary to guard against it, is well illustrated by the

many outbreaks which occurred in various parts of the country during the past summer.

The report of Dr. Salmon also shows very clearly that the infection of Texas fever is advancing over new territory; that many square miles have been overrun where the winters are quite severe and where snow and ice are by no means uncommon. Heretofore it has been supposed that this degree of cold would infallibly destroy the contagion and prove an insurmountable barrier towards its becoming permanent in the more important cattle-raising sections of the country.

From these facts it became very desirable to learn, as nearly as possible, the present distribution of this disease, the rate at which it is advancing, and the means by which it may be controlled. With the aid of a number of assistants, Dr. Salmon has succeeded in tracing the line which at present separates the infected from the uninfected parts of Virginia for many miles, and has located this as definitely as possible. The map which accompanies his report shows at a glance the boundary line of the infected territory, so far as traced, and also conveys a sufficiently exact idea of the geographical distribution of the disease throughout the State to demonstrate the importance of this investigation.

The evidence to show that the disease in Virginia is identical with Texas fever is considered at length, while much information is furnished bearing upon the value of the animals lost and the manner in which the disease is spread.

The scientific investigation in regard to the nature of the disease has not been neglected. Successful inoculations were made with splenic pulp, which confirm the results of the experiments of 1881. There can no longer be any doubt of the inoculability of this disease when the proper liquids from the spleen are used.

The splenic pulp has been carefully examined with a view of isolating the contagious principle of the disease. A form of micrococcus appears to be the only living thing revealed either by the microscope or by the cultivation experiments; and, although the inoculations with this cultivated organism were not successful, it is not improbable that this is the true germ of the disease. The blood in the cases so far examined seems to be entirely free from virulent properties, and also free from the micro-organism found in the spleen.

A number of facts were brought to light which indicate that susceptible cattle contracting this fever on permanently infected grounds may, in certain instances at least, be the bearers of the contagion and infect fresh pastures to which they gain access. This is an exception to the rule that sick cattle are harmless, which is of considerable importance to cattle owners in the vicinity of the infected district.

The rate at which the infection is advancing over fresh territory has also received much attention. This varies greatly in different localities, in some cases being as much as four miles per annum for a long series

of years, while in one or two instances there has been no perceptible advance for half a century.

The investigations of fowl cholera show that no degree of immunity is inherited by the offspring of fowls made insusceptible to this disease by vaccination. The attempts to produce insusceptibility by means of devitalized virus have so far been without any positive results.

The inoculations with diluted virus have been continued and confirm the conclusions reached in the preceding report. Inoculations with a dilution of 1 to 125,000 produced no effect, but the same birds reinoculated with a dilution of 1 to 100,000 contracted the local inflammation, which indicates the success of the operation. Dr. Salmon thinks that an inoculation with a dilution of 1 to 70,000 would be successful with nearly half of the birds operated upon, and that the remainder might then be reinoculated with a dilution of 1 to 10,000, which would be sufficient to protect all. The duration of the immunity obtained in this way appears to be sufficient to protect birds during their whole lives.

Many experiments were made to test the exact strength of disinfecting solutions necessary to destroy fowl cholera virus in a given time, for the details of which the reader is referred to the body of this report.

An exhaustive review of the different methods of attenuating virus for vaccinating purposes is a conspicuous feature of Dr. Salmon's report. While admitting the attenuation produced by Pasteur, and the importance of the discovery, it is argued that the attenuations of Pasteur are due to the loss of activity incident to the old age of the germs, and not to the direct effect of atmospheric oxygen. The subject is one of great importance and the data bearing upon it are fully discussed.

A comparison of the different methods of attenuating virus reveals the fact that those discovered by Pasteur and Salmon are the only ones likely to be of general applicability in this country. A more extensive investigation of these methods will be necessary before a decision can be reached as to their relative merits.

In conclusion, Dr. Salmon reviews the most destructive of our animal plagues and shows that with hog cholera, fowl cholera, black leg and charbon, vaccination is the only means by which we can prevent their ravages. He thinks vaccination is plainly indicated for the prevention of these diseases, and that the vaccine should be prepared and distributed by the Department of Agriculture. Important suggestions are added in regard to the investigation and control of other diseases.

The measures recommended for checking the advance of Texas fever will be read with great interest by those interested in the cattle industry in all parts of the country, and will doubtless receive attention commensurate with the importance of the subject.

The report of the results of Dr. Detmer's investigation of southern cattle fever will be found interesting and instructive. He made a number of *post-mortem* examinations of animals that died of the disease, and used some remedies with partial success. His experiments were

conducted on the frontier, and owing to the absence of facilities he made but few attempts to cultivate the virus. The symptoms and *post-mortem* appearances of the malady, however, were thoroughly studied, and the foundation laid for a more searching examination during the current year. From his experience with the disease thus far he is of the opinion that it is purely an infectious fever, and does not think it is ever communicated by mere contact.

The infectious principle requires a medium or vehicle outside the original organism in which to develop its malignancy, and through which only it is able to cause an infection of an animal that does not possess immunity. After causing an infection of a susceptible animal it apparently loses its malignancy or ability to transmit the malady further.

The period of incubation is uncertain. Its duration seems to depend more upon the amount or quantity, and particularly upon the intensity or malignancy of the infectious principle imparted, than upon the individuality of the animal. Usually one or two animals show plain symptoms a day or two earlier than the others; then several animals are taken sick at the same time, and generally within a week or ten days after the appearance of the first case the whole herd, with perhaps the exception of one or two animals which appear impervious to the disease, will show characteristic symptoms of the fever.

Northern cattle when taken to the south and pastured on thoroughly infected ranges usually show the first symptoms within two or three weeks after their arrival.

Dr. Detmers thinks the immunity possessed by southern cattle is only a temporary one. Southern or Texas cattle that have spent but one winter in the north, even if only as far north as Southern Kansas or the northern portion of the Indian Territory, lose their immunity, and are just as susceptible thereafter as any other cattle.

Much valuable information relating to the early history of southern cattle fever in North Carolina and Virginia will be found embodied in the report of Dr. J. M. Hines. This report contains the enactments of the colonial and subsequent legislatures of the last named State, adopted with the view of preventing the spread of the disease.

This fever was more wide-spread and destructive last season than for many years past. On another page will be found many telegrams and letters giving the Department information of outbreaks in different localities and asking the Commissioner for aid in their suppression. Veterinary inspectors were at once dispatched to most of these points, and through their timely advice many valuable animals were saved. Special report No. 50 was also issued and widely distributed in those localities in the northwest where southern cattle are usually grazed in the summer and fall months.

In addition to his experiments with southern cattle fever, Dr. Detmers devoted considerable attention to the investigation of diseases which have heretofore proved very destructive to sheep. A thorough

investigation of the malady generally known in Texas and throughout the Southwestern States as "lombritz" was made, its character and cause determined, and a successful remedy discovered and applied. A careful persual of the report of Dr. Detmers on this important branch of his investigation will enable the flockmasters of the Southwestern States to readily diagnose the malady, and a compliance with the preventive and remedial measures recommended will no doubt result in a speedy extinction of the disease wherever it may be found prevailing.

The statistical correspondent in London furnishes the Département with information concerning the existence and spread of foot-and-mouth disease (aphthous fever) among the cattle of Great Britain, and the efforts made to hold our shippers responsible to some extent for the increase of the malady on that island. There is really no foundation for this charge, as the disease has never prevailed to any extent on this continent. Some two years ago the steamship *France*, from Liverpool, landed some cattle in New York that were found to be infected with the disease. These animals were quarantined until the disease disappeared, and no infection to other cattle ever resulted from their importation. In March last the steamship *Nessmore* landed forty head of Guernsey cattle at Baltimore which were suffering with the contagion. They were at once placed in quarantine, where they still remained at last accounts. They did not come in contact with any other animals, and not a case of the disease has yet resulted from their importation. The steamship *Nessmore* was not disinfected. On her return trip she carried a cargo of cattle, many of which were found to be suffering with foot-and-mouth disease on her arrival at Liverpool, they having contracted the malady from the contaminated vessel during her return voyage.

The reports of Drs. Miller and Rose give detailed statements of inspections of cattle made during the year. It will be seen from these reports that contagious pleuro-pneumonia or lung plague is less prevalent in the infected districts than it has been for several years.

In addition to the above, an excellent paper on the subject of contagious diseases of domestic animals, and the duty of the Government as to their suppression, from the pen of Dr. Ezra M. Hunt, secretary of the State Board of Health of New Jersey, together with many interesting extracts from letters of correspondents, will be found in this volume.

INVESTIGATIONS OF CONTAGIOUS DISEASES DURING THE YEAR 1882.

INVESTIGATION OF TEXAS CATTLE FEVER AND FOWL CHOLERA.

REPORT OF D. E. SALMON, D. V. M.

Hon. GEO. B. LORING,

Commissioner of Agriculture:

SIR: I have the honor to submit a report of the investigations, made by me during the past year, in regard to the nature of the contagious and infectious diseases of animals, and the remedies which are applicable to them. These investigations have been confined to the two diseases known as Texas or Southern Cattle Fever, and Fowl Cholera; and they have been pressed forward as rapidly as possible, considering the difficult nature of the subject and the many obstacles which have been encountered. In the following pages I have endeavored to so arrange the information obtained, and the accounts of the investigations, as to give a connected view of each part of the subject, and to bring out the bearing of the facts both upon these particular diseases and upon the general subject of contagion.

TEXAS CATTLE FEVER.

The investigations of this disease have been carried on from two different standpoints: First, to learn the present outlines of the permanently infected district, the rate at which this district is enlarging itself, and the amount of losses which annually occur along the slowly advancing line; secondly, to learn the nature of the disease, the character of its contagion, the manner in which this is distributed, preserved and taken into the system, and, finally, the means by which it can be successfully combated. The facts bearing upon the first part of this inquiry were mostly collected by Major R. C. Saunders, of Evinston, Va., who, as well as his assistants, Messrs. Benjamin Hubbard, R. C. Saunders, jr., and W. B. Shaw, has worked most intelligently and perseveringly in tracing out the many isolated cases and details which were necessary to establish the line of the infected district with any degree of certainty.

While directing in a general way, the work of these gentlemen in Virginia, I have kept up the scientific investigations and accomplished as much as was possible under the circumstances. But the results of the year's work show very plainly that this disease is one which can-

not be readily investigated without more facilities than have heretofore been at hand. The reason for this conclusion, as well as the proper manner of conducting the investigation, I hope to make apparent in the following pages.

The permanent home of the disease known as Texas fever was formerly believed to be confined to the Atlantic coast, south of North Carolina, and the Gulf coast from Florida to Texas; it was supposed to be the result of the climatic influences of this peculiarly malarial district, and as, when carried to the North, it had always disappeared with the first frost, the teaching has been general that its contagion could never exist through the winter beyond the line of frost and snow. Having already, in former reports, given many facts tending to show that a part of Virginia, nearly all of North Carolina east of the Blue Ridge, and the greater parts of the States of Georgia, Alabama, Mississippi and Louisiana were long since overrun with this plague, I proposed to call it the Southern Cattle Fever, as this would be a much more appropriate and less misleading title than Texas fever. The name, however, cannot be of much consequence as long as we understand the facts which I have already referred to; and as many of these facts are widely and, I might say, almost universally contested by those who pretend to understand this disease, I shall enter into the details at considerable length in regard to the evidence on which my conclusions rest.

Just here, it is necessary for me to remind the reader of certain characters of the disease which have been long known and are quite generally admitted, and which cannot be ignored if we would appreciate the bearing of the facts presented in this report. In the permanently infected district, and, to avoid even the remotest appearance of error, I refer at present to the Gulf coast from Florida to the Rio Grande, there are no remarkable losses among the native cattle from this disease. On the contrary, the cattle are quite as healthy and look as well as we could expect, when we consider the scant and unnutritious herbage on which they exist, the utter neglect which they experience through all seasons of the year, the excessively hot and dry summers, and the abundance of insect pests. Whether the stomachs of such cattle are abnormally reddened and present unusual signs of former ulcerations, or whether their spleens and livers are greatly enlarged, as Professor Gamgee seems to have demonstrated, it is foreign to my present purpose to inquire; but what I wish to dwell upon is the fact that there is no great mortality among the cattle in this district that can be attributed to this disease. Nevertheless, we call it an infected district, because when we take cattle there from New York, or Ohio, or Illinois, such cattle almost invariably contract a disease having all the characters of Texas fever; and to make sure that this is not the result of climatic influences, properly speaking, we have only to bring the native cattle from the Gulf coast among the herds of the North to communicate the same disease with all its symptoms.

The fact that the Gulf coast is infected with this disease is not shown,

then, by any effect on the cattle native to that region, either when they are on their native pastures or when they are moved to the North, for in both cases they retain their condition of apparent health; but it is shown by the death of susceptible cattle taken to that section, and by the ability of the native cattle to carry the germs of the plague for hundreds of miles and to thus communicate it to the cattle with which they come in contact. I insist more particularly upon this fact because many who regard it as sufficient evidence that the Gulf coast is infected with a specific disease are either doubtful or altogether skeptical when it is applied to regions that are further north. But, when we come to examine into the matter, we find that there are just these two classes of facts, and no others, upon which we can rely to determine whether any given territory is permanently infected with the Texas fever contagion. If they are insufficient in regard to Virginia, they are equally insufficient in regard to the Gulf coast and any part of Texas; consequently, there would be no section infected with Texas fever, and, if not, there certainly could be no such disease, and the whole argument leads to a *reductio ad absurdum*. But Texas fever is a terrible reality; there are sections permanently infected with its germs; we know this by the kinds of evidence I have mentioned, and as we do not know the outlines of these sections we must establish them according to this same evidence. And so, when I have established the fact that cattle from the North, when taken to any particular county or town in the South, are almost invariably affected with the symptoms of Texas fever during the ensuing summer and fall; or when it has been shown that the cattle, from such a county or town, when taken further north disseminate a plague with similar characters, and, particularly, when both of these facts are discovered, I shall consider that county or town to be permanently infected with Texas fever.

There is no doubt that it is a difficult matter to understand, in the present condition of science, how it is possible for the native cattle of a section permanently infected with a contagious plague to resist the influences of the contagion with which they are surrounded; it is equally difficult to understand how apparently healthy cattle can distribute this contagion for so long a time after they leave the infected district; and it surely is not less difficult to understand why the cattle really sick of this contagious disease do not convey the contagion to others. But these facts are perfectly apparent from hundreds and thousands of observations, and they are just as true of the disease when it originates on the Gulf coast as when in North Carolina or Virginia; and, consequently, they cannot be accepted as tending to show that any district is uninfected so long as its cattle carry contagion beyond its borders, or so long as susceptible cattle die upon its soil.

DISTRIBUTION OF TEXAS FEVER IN VIRGINIA.

For several years I have been convinced, from isolated facts gathered here and there, that a section of territory in the southeastern part of

Virginia was permanently infected with Texas fever. Just how large this territory might be, or how thoroughly the infection was disseminated over it, were questions which I was unable to decide, and on consulting the State authorities of Virginia, and many of the best informed and most intelligent of the stock-raisers, I found that there was a general and most profound ignorance of the whole subject.

The high price of cattle and the greatly increased number of these animals now being fed by the Virginia farmers has greatly stimulated the traffic and led to the purchase of young animals at a greater distance from home than ever before. But with the revival of this industry came the most terrible losses. Cattle feeding upon pastures where such animals had always before thrived would commence to droop and die, until 30, 50, and 75 per cent., and, in some instances, every animal, had perished.

While a few of the best-informed agriculturists of the State believed the trouble to be caused by the "distemper," or, as some called it, the "North Carolina distemper," there were not many who suspected its identity with the Texas fever, and by far the greater part of the interested farmers attributed their losses to some mysterious change in the atmosphere, the soil, or the character of the vegetation.

When, therefore, in accordance with your instructions, I visited Campbell and Bedford Counties, in the latter part of July, I found the cattle owners of a large district in the greatest consternation and alarm. Cattle were dying on every side. Men who had invested their all in stock with the hope of realizing largely because of the high prices, saw their capital melting away before their eyes and were powerless to prevent it. Great numbers of farmers were losing the small savings which had been accumulated by years of labor. On every hand was uncertainty as to the cause, how it had come, how it might be checked, and how prevented in the future.

A few days' study of the characters of the outbreaks, of the symptoms and *post-mortem* appearances of diseased animals, were sufficient to demonstrate the identity of the plague with that which has long been known to the veterinary profession as Texas or Spanish fever. But no Texas or Gulf coast cattle had been brought here; even North Carolina cattle had been avoided, because there was a tradition that these were dangerous, and nearly all the trouble appeared, from the superficial investigation which I was able to make at the time, to proceed from animals purchased from the neighboring counties on the South.

At a public meeting of those most interested in the cattle industry of this section of the State, I pointed out the nature of the disease and the fact that the cattle had contracted it by grazing on pastures infected by animals from further South, and that if all susceptible animals were at once moved to pastures where none of these foreign cattle had been no further infection could occur; that, even, if cattle already infected should

sicken and die on the fresh pastures, that would not affect the remainder, for the reason that such sick cattle had never been observed to infect either other animals or the grounds upon which they ran; that native cattle, which had so far been kept from pastures on which the dangerous stock had traveled, were perfectly safe as long as they were kept upon such pastures; and, finally, that cattle could not be brought even from a few miles to the south, except, perhaps, in December and January, and allowed to travel over roads, commons, or pastures without the greatest danger to their native stock.

With these assurances much of the alarm disappeared; the mystery was cleared up; people saw what could be done with a hope of checking the plague; cattle which had not been exposed were kept rigidly isolated. And from this information, as the owners assured me, were saved some of the most valuable thoroughbred herds of cattle in the State—herds which had been collected, bred and acclimated by the labor of a lifetime, and which money could not replace.

This was about all that could be done for the immediate suppression of the disease. The infected pastures would remain infected till purified by the frosts of the coming winter; the infected cattle would develop the disease in spite of precautions, and the diseased ones would not be greatly benefited by any known treatment. But another question of greater importance presented itself. A part of Virginia was without doubt permanently infected with Texas fever, and the cattle from that part were as dangerous as those from the Gulf coast, more dangerous; in fact, because there were so few who suspected them. The outlines of this section were unknown, and while such was the case, it was impossible to say where cattle could be bought with safety. The State could make no laws to control the movement of dangerous cattle nor to prevent the permanent extension of the plague, while people in other States had no opportunity to know that by buying cattle in this section they might destroy not only their own herds but those of a whole community. Again, there were facts which made it certain that the district permanently infected with Texas fever was gradually becoming larger, that the boundary line was moving farther and farther towards the north, and that within this line neither the frosts nor snows of severe winters were sufficient to exterminate the infection. It was of the greatest importance for us to learn the rapidity of this advance, and to collect, if possible, the materials which would indicate what measures might be adopted for arresting it.

To obtain this information required an enormous amount of work, and the patient collection of isolated facts which no one man could accomplish. It was for this reason that I appealed to the Department for those assistants who were so readily granted, and whose intelligent and careful inquiries enable me to rest my conclusions upon a much firmer basis than would otherwise have been possible.

LOSSES IN PREVIOUS YEARS.

It was not a leading object in this investigation to collect statistics of the many outbreaks of Texas fever that have occurred in Virginia during past years, unless there was a probability of these throwing some light upon the boundary of the infected district, the rate at which this boundary was advancing, or the measures for controlling the disease. The few cases referred to must not be taken, therefore, as a complete history of the disease in this State, but simply as examples of what has been occurring for years past over a large extent of territory.

In answer to inquiries, Maj. R. L. Ragland, of Halifax County, sent me the following valuable information, which I give in his own words:

I have never seen anywhere a more correct description of the cattle disease, which prevails more or less every year in this and many of the adjoining counties in Virginia and North Carolina, than is given on pages 253-255 of Department report on "Diseases of Swine and other Domestic Animals, 1879." I have no means of tracing its history ("distemper" as it has been called for more than half a century), but all authentic accounts agree that the disease was brought from Eastern North Carolina to Virginia—from the pine and swamp region of North Carolina, as stated by Mr. Lenoir, where it existed long prior to its appearance in this State. The disease was prevailing here when I was born, over fifty years ago, and prevails more or less every year. The cause of the disease is not known. It came from Eastern North Carolina sixty or seventy years ago, and progressed westward very much as stated by Mr. Lenoir.

One remarkable fact connected with the disease is that there are farms on which a case of the "distemper" has never occurred, while on the farms adjoining it kills more or less of the cattle every year. It is more prevalent and violent on some farms than on others. It rarely makes its appearance before July in this section, usually comes with *hot*, dry weather, and is most prevalent and fatal in August.

No known remedy can be relied upon. Red clay, salt, sulphur, saltpeter and copras, when properly mixed in right quantity, act as a preventive unmistakably.

In Washington and Russel Counties, the great stock-raising region of Southwest Virginia, there have been a number of outbreaks from imported cattle, but these have left no permanent infection. Thus, in September, 1867, 70 head of Texas cattle were pastured for ten days on the farm of Mr. Henry Preston at Wallace's Switch, Washington County. Six head of natives were running on the same pasture, but only two contracted the disease. These were valued at \$50. All the native cattle on the farm became covered with the ticks, which so many think are the cause of the trouble, but none of those grazing in other fields showed any signs of disease.

Again, in May, 1868, 125 head of cattle were purchased by a Mr. Cunningham, near Memphis, Tenn., and were driven along the road by Mr. Preston's place. Mr. Preston's native herd of 65 head were driven along this road at various times from one pasture to another, but were never allowed to stop. The last of July the disease broke out among them and three died, which were worth \$80. All the native cattle again became covered with ticks.

I mention these facts in regard to the ticks, because so many still cling to the theory that they are the inducing cause of the disease;

and, yet, it is clearly seen from these cases that although all the native cattle were covered with ticks, but two died in the first instance and three in the second, and in either case only those which had been upon infected roads and pastures.

In the fall of 1868, 150 head of cattle were purchased in Texas, shipped to Arkansas and wintered, and about the 1st of May were brought to Bristol. The 6th of May they were turned out on a common near that town for about eight hours, thoroughly infecting it and causing a loss of 50 milch cows, valued at \$1,500. They were then driven beyond the salt works, in Smyth County, infecting the road over which they traveled, and causing the death of 30 more cows, valued at \$900. They passed through the Salt Works farm along a plantation road, and spread infection which destroyed 30 thoroughbred cattle, worth at least \$1,200. This single drove of cattle, therefore, destroyed \$3,600 worth of native animals.

In 1878 about 70 head of cattle were purchased in Marengo County, Alabama. These stopped for 36 hours at Mr. Preston's farm, at Wallace's Switch, the last of May. Mr. Preston's cattle commenced dying the 10th of August, and he lost 30 head, valued at \$1,500. In this case, too, all the native cattle were infested with ticks; and the calves which sucked their mothers up to their death were not themselves affected. This drove was taken to Glade Spring, and by infecting the roads and pastures along the route and at their destination caused the loss of 50 other cattle.

In the latter part of April, 1880, a drove of cattle was purchased in the neighborhood of Gainesville, Ala., and shipped to Bickley's Mills; Russell County. We have reports from five parties who together lost 59 animals from their infection, valued at \$1,287.

In none of these instances have we been able to secure details of all the losses, but what we have presented are sufficient to demonstrate that all these counties are free from any permanent infection, first, because the native cattle proved so susceptible to the infection, and, secondly, because no losses have occurred after the succeeding winter in any case.

I now pass to a consideration of the evidence which establishes the outline of the infected district in Virginia. And, by way of explanation, I would remark here that as the infection has progressed towards the north and west, all that part of the State south and east of the line, which we have traced, may be regarded as permanently infected with the so-called Texas fever. While this is practically true, however, it is not absolutely true, for there are many farms, possibly, also, some neighborhoods, which have not yet become infected. For this reason every lot of animals from this section do not carry the infection, nor do all the animals brought from the north or west of this line invariably die. That the movements in both directions are extremely dangerous, however, is proved beyond the possibility of doubt.

REPORTS FROM PATRICK COUNTY.

This is one of the southern tier of counties, and is situated immediately east of the Blue Ridge.

Our first report is from a farm $1\frac{1}{2}$ miles from the North Carolina line and 7 miles east of Carroll County. Cattle were lost here of this disease in 1872, and nearly every year since. Another report from a farm a mile farther west says that cattle have been lost more or less every year since about 1852. Still another report from a farm at the base of the Blue Ridge, 5 miles from the North Carolina line, tells us that the pastures and roadsides have been infected for twenty-five years, and that cattle have been lost on this farm for the last ten or twelve years. All the intelligent people of this section are of the opinion that the disease came from North Carolina, and has gradually extended northward. A gentleman, who lives 12 miles from Carroll County and 5 miles from North Carolina, bought five head of cattle from the top of Willis Gap, in March, 1882. Two of these died in May from Texas fever. The pastures at this place had been infected since 1872.

Coming now to Patrick Court-House, we have a number of reports from a section situated from 7 to 10 miles from the North Carolina line, and from 20 to 25 miles east of Carroll County. About \$300 worth of stock had died from the disease this year, without any introduction of foreign cattle. All reports agreed in stating that the pastures here had been infected for about thirty years.

From Meadows of Dan, in the northwestern part of Patrick County, we have reports of three outbreaks caused by cattle brought, in one case, from Patrick Court-House, and, in two cases, from a section 15 miles west of Patrick Court-House and 4 miles from the North Carolina line.

There can be no doubt, then, that a considerable portion of Patrick County is permanently infected, that this infection has occurred within the last thirty years, and that cattle taken from this infected territory may infect the pastures on which they run.

REPORTS FROM HENRY COUNTY.

This county is situated directly east from Patrick, and, also, borders on the North Carolina line.

At Ridgeway the pastures have been infected for more than thirty years; at Horse Pasture, Spencer's Store, and Irisburg there is no doubt of the permanent infections, but we have no facts showing how long it is since this occurred. Going farther north we find that in the neighborhood of Burnt Chimneys, Mountain Valley, and Leatherwood the permanent infection has been of very recent occurrence. At Burnt Chimneys the roadsides have been infected since 1879; at Mountain Valley the pastures were infected from 1879 to 1881, and at Leatherwood some pastures were infected in 1881, others not till this year.

A large number of cases of the disease have been investigated in the section of country between Martinsville and Traylorville along Smith's River, and these show, very conclusively, that the infection is slowly advancing up this river, being about a mile farther on the south than on the north bank. Even in the sections which have been infected thirty or forty years, the native cattle are still dying every year in considerable numbers. This loss occurs principally among cattle running on the commons, while those on inclosed pastures from early spring till late fall generally escape.

We have one report showing that cattle, taken from Henry County to Tazewell, carried the infection and caused the death of 17 animals, valued at \$350. This gives both lines of evidence proving the permanent infection of the county.

REPORTS FROM PITTSYLVANIA COUNTY.

This county also borders on North Carolina, and is immediately east from Henry.

The losses of cattle have been enormous over most of the county for the last thirty or forty years, and continue to be extremely heavy. We have investigated cases of the disease in forty-six different localities, and all go to prove that the whole county has been overrun with the permanent infection. Our facts in regard to the rate at which the disease has advanced in this section are not as numerous or as accurate as we could wish, but they indicate that about 30 miles has been covered in twenty years. At present this advance has been checked by the Staunton River, which divides Pittsylvania and Campbell Counties.

Cattle from Pittsylvania have caused at least three outbreaks in Bedford and as many in Campbell County the present year, so that here again we have both lines of evidence demonstrating the permanent infection.

REPORTS FROM FRANKLIN COUNTY.

This county is directly north of Patrick and Henry and is west of Pittsylvania.

Here, again, the infection has followed the water-courses and advanced with very considerable rapidity. It has gained 12 miles on Snow Creek within three years, and has reached commons on Pig River 8 miles from the Pittsylvania line. This is abundantly demonstrated by the death of cattle running on such commons which had no opportunity for infection by foreign animals.

Four miles southeast of Union Hall the roadsides and commons have been infected for the last four years; and cattle taken from there to Union Hall this year carried the infection, and have undoubtedly caused permanent infection of the commons at that place.

There is, then, but a comparatively narrow district in the southeastern part of the county which up to this time has been permanently in-

fectured with Spanish fever. The losses during the year, so far as ascertained, were sixty head, valued at about \$1,400.

REPORTS IN REGARD TO HALIFAX COUNTY.

I have already given the very interesting letter of Major Ragland, which shows that the southern part of this county has been infected for more than half a century. In 1880, a gentleman living at The Forks, purchased nine head of cattle in Campbell County and took them to his farm. Of these six died, having commenced to sicken in April, as the result of grazing on permanently infected pastures.

In 1872 a lot of over 100 cattle was bought in the northern part of Halifax County, between Republican Grove and the Staunton River, and in the neighborhood of Brook Neal. These cattle were taken to Bedford County, and there infected a stable-yard belonging to Mr. David Kyle, from which 26 fine Short-Horns contracted the disease and died—a loss of \$5,000.

In August, 1882, two head of cattle which were purchased in Halifax County were taken to the farm of Mr. Chapman Glover, at Glenmore, Buckingham County, where they infected his pastures and caused the death of nine natives. About the same time, 22 cattle from Halifax were taken to the farm of Mr. Howard Lewis, at Scottsville, Albemarle County, and infected his pastures, causing the death of three native animals.

A herd of 80 head at Green Hill, Campbell County, had been wintered in Halifax, just across the river, for a number of years. Last spring, probably from being moved too late, or because of the mildness of the winter, they brought the infection home with them, and twenty-three died during July and August.

These facts are sufficient to indicate that this county is infected throughout its whole extent; indeed, this was so evident that very little time was spent in investigating the many cases of the disease which occurred there during the year.

REPORTS FROM CAMPBELL COUNTY.

In the neighborhood of Brook Neal, in the extreme southeastern part of the county, cattle have been dying of Spanish fever nearly every summer for more than fifty years; and it has been well known that animals from the North or West were nearly sure to die. Cattle have died here the past summer, and a sufficient number of cases were investigated to satisfy us that the pastures are permanently infected. The infection has gradually extended in a northwestern direction from this as a starting point, being limited on the east by Falling River, and on the west by Whipping Creek. In 1881 the commons about Pigeon River were poisoned, and they remained in this condition through the winter. A number of cases were investigated here, which show, only

too plainly, that the border line of the infected district has now reached this point. The extension of the disease here has been very slow, probably because the infected territory was so narrow, and but 10 miles has been covered in fifty years. What makes this extension of greater significance is, that the Staunton River, which has held the disease in check, heretofore, and continues to do so to the west of this point, has here been crossed, and there is nothing now to stop the steady advance until the James River is reached.

REPORTS FROM CHARLOTTE COUNTY.

Our information from this county is very meager, and we have not the data to establish the line of infection across it in anything like a satisfactory manner. All of the northern and western parts of the county are evidently free from any permanent infection. The river has here held the disease in check, and caused the border line of the district to turn very considerably toward the south. The indications are that the infection is confined to the immediate vicinity of the northern bank, until we reach a point a few miles below the crossing of the Richmond and Danville Railroad. From here it has extended beyond the neighborhood of the river, the line of the district being apparently nearly parallel with the railroad and but two or three miles from it. At Mossing Ford and Keysville, on the line of the railroad, the losses have been quite severe during the summer, and it is probable that the commons at these places will remain infected in the future.

PRINCE EDWARD AND LUNENBURG COUNTIES.

We have no reliable reports from these counties. The line of infection, which becomes obscure as we leave the Staunton River, is here entirely lost, for the present, and we can only indicate it by conjecture. The greater part, if not all, of Lunenburg would seem to be infected, though it is possible that the extreme northwestern section is still free from it. Prince Edward is probably only infected in its eastern or southeastern part. Just as we were endeavoring to clear up the line across Charlotte, Lunenburg and Prince Edward Counties, our inspectors were forced to their beds by malarial fever, and the work stopped. It was a great disappointment to have this part of the line left in so uncertain a condition, but the investigation can be resumed at any time.

REPORTS FROM BUCKINGHAM COUNTY.

The line of infection, lost above the Staunton River, becomes plain again at about the point where the Willis River crosses the dividing line of Cumberland and Buckingham Counties. Gravel Hill and Gold Hill are points which have been long infected. Diana Mills seems to be on the border line, and has been more recently invaded. This line, leaving the Willis River near where it crosses the county boundary, passes about

3 miles west of Gravel Hill, and from there leads nearly directly to Diana Mills; from this point again it leads down the west bank of the State River to the James.

REPORTS FROM FLUVANNA AND GOOCHLAND.

We have a number of reports from these counties, and, although many cases of Spanish fever occur in them from time to time, they have so far all been traced to cattle arriving the same summer from the infected districts south of the James. According to the best information at hand these counties are free from any permanent infection, the progress of the disease being still arrested by the river.

REPORTS FROM HENRICO AND HANOVER COUNTIES.

From many conversations with cattle owners about Richmond I have no doubt that the north bank of the James is infected at this point. The line of infection, according to the information at hand, leaves the north bank of the James some distance above Richmond and crosses Henrico County. We have not been able to closely investigate the many cases of disease which have occurred in this county during the year, and nothing short of this will enable us to locate the line with certainty.

In Hanover County, we know that Ashland and Hanover Court House and the sections surrounding these towns are permanently infected, though there seems to be a doubt if the cattle from here can infect susceptible animals when carried west or north. This probably arises from the recent infection of the district, and the consequent fact that many farms are still free from it; of course cattle from these would carry no contagion. The line of infection evidently strikes the Pamunky River just above Hanover Court House, but beyond here we have not sufficient information to locate even a conjectural line.

President Conrad, of the Virginia Agricultural and Mechanical College, who is the chairman of a committee appointed by the Life Members' Association of the Virginia Agricultural Society to assist in this investigation, has kindly written me as follows in regard to the doubtful district north of Hanover County:

I learn that in Hanover County, above tide-water, and within a few miles of Ashland, the farmers will not allow the cattle from King William, King and Queen, Essex, and adjoining counties to be brought among their native cattle, nor will they allow their cattle to pasture in what is known as the commons, for reason that disease is thus contracted and death the result. So you need not consider these counties lying between the York and the Rappahannock Rivers as any longer *doubtful*, but as certainly to be classed in the group of infected and diseased counties.

It would appear, from this information, that a line drawn from Hanover Court House, in a northeasterly direction, along the boundaries of King William and King and Queen, across Essex to the Rappahannock, would not fail to any great extent in separating the infected from the

still healthy territory south of this river. Has the infection crossed the Rappahannock and invaded the four eastern counties lying between this river and the Potomac? It is not improbable; but we have absolutely no information, from this section, at the time of writing this report.

In our investigations of the extent of the infected part of Virginia, we were continually being surprised at the advance which the infection has made. We were prepared to find the southeastern counties infected, but we did not expect to find this true of counties so far west as Patrick and Henry. Nor did we expect to find permanent infection in Buckingham, Cumberland, Amelia, Powhatan, or Chesterfield, and, still less, in the counties north of the James River.

The truth, then, as brought out by the investigations of this year, demonstrates that we really underestimated the importance of the disease and the extent of its advance, in our former report, instead of greatly exaggerating both, as was assumed by some who felt called upon to criticise our conclusions. The facts in regard to the infected district and its gradual extension, which I have briefly summarized above, are sufficiently plain to need no comment. The importance of the matter is seen at a glance, and nothing which I can say would make this more evident, or render the necessity of controlling legislation more apparent to the most superficial thinker.

EVIDENCE THAT THE DISEASE IN VIRGINIA IS IDENTICAL WITH TEXAS FEVER.

As it has not been heretofore suspected that the disease affecting cattle over so large a portion of Virginia, North Carolina, and other Southern States was identical with Texas or Spanish fever, and as many will doubtless feel inclined to contest this conclusion, I will briefly review the evidence on which it is based.

In the diagnosis of a contagious disease, there are three sets of facts to be taken into consideration: 1. The general characters of the disease as manifested by the classes of animals affected, the extent of the outbreaks, and the peculiar ways in which the contagion is spread. 2. The symptoms of the disease as observed with affected animals. 3. The appearance of the internal organs after death.

Fortunately, the investigations made by Professor Gamgee and by the Metropolitan Board of Health, in 1868, were so complete in regard to these points, as to leave nothing to be desired. We learn from them that Texas fever is in many respects a very peculiar disease, differing in its general characters, its symptoms, and its *post-mortem* appearances to a remarkable extent from any disease described in the veterinary literature of any part of the world. The native cattle in the infected districts seldom, if ever, suffered from it; milch cows, fat cattle, and working oxen were the classes of animals generally affected, while calves as a rule escaped; the disease was spread by apparently healthy cattle,

and these cattle infected pastures for weeks and months after leaving their native country; it was only contracted from infected grounds; sick animals seldom, if ever, spread the contagion; a fence was sufficient to arrest the disease; the only kind of cattle that could be imported into the infected district with any safety was young calves; the disease almost invariably occurred in summer and fall, and was arrested by a frost. Surely this list of characters is so extraordinary, so different from what is seen with any other disease, that one could not hesitate to diagnose the affection from these alone, if he knew nothing of the symptoms or *post-mortem* appearances.

The symptoms taken by themselves are not so characteristic. The lopped ears, drooping head, staggering gait, high temperature, and bloody urine, are seen with anthrax, and perhaps one or two other diseases. The duration of the disease is, however, three or four times as long as with anthrax.

The *post mortem* appearances are better indications than the symptoms. The enlarged spleen and liver and the engorged kidneys are also seen in anthrax, but the erosions and ulcerations of the stomach are characteristic, and these with the complete absence of the gelatinous exudations of anthrax are sufficient to exclude this disease. A still more important difference is the absence of the *Bacillus anthracis*, or parasitic bacterium, now demonstrated to be the cause of anthrax.

Now, if we place together the general characters, the symptoms and the *post-mortem* appearances, we have so peculiar and distinct a combination that, when all are considered, it is impossible to mistake the disease. No veterinarian could hesitate for a moment in his diagnosis if all of these distinguishing characteristics were placed before him.

Very well; these characters and symptoms have been recounted to us, again and again, all along the line of the infected district in Virginia from the North Carolina line to the Rappahannock River. In regard to the *post mortem* appearances, we have assured ourselves by many examinations, we have studied the blood and other liquids and solids under the microscope, and we feel that there cannot be a suspicion of doubt as to the disease which we have been engaged with.

Prof. John R. Page, of the University of Virginia, has kindly sent me the notes of a *post mortem* made on a cow at Charlottesville, in which the lesions were so characteristic that I insert them in full with the accompanying remarks:

The subject was a large red cow resembling a short-horn grade, weighing over a thousand pounds, and said to have been in good condition previous to the fatal attack of disease. The animal, with a calf one month old, was purchased the 21st of September in the old stage yard in Charlottesville. While there a lot of cattle were driven into the yard, and it is said that there were diseased ones on the farm from which they came.

Symptoms.—The first symptoms of disease were manifested on the evening of October 6. There was sudden, almost entire loss of milk, loss of cud, and very high fever, drooping head and eyes glassy and fixed, urine lessened in quantity, deep-red or black

in color, dung hard and lumpy. The fever and constitutional weakness became extreme, and the animal was in a state of frenzy, passing into stupor before death, which occurred on the night of October 9th.

Examination twelve hours after death.—The paunch and intestines were distended with gas, and the fat on the surface of the whole was tinged with a bright saffron yellow color. The blood vessels were filled with dark purple blood (congested). The paunch filled with undigested food, and the mucous membrane lining it black and decomposed. The maniplus or book was filled with a large quantity of undigested food between the folds or leaves, which was very dry, the mucous membrane being also black and mortified. The true stomach or abomasum was partially filled with fluid food-material, and its lining membrane was covered with dark purple mortified patches, running into each other all over the surface. The lining membrane of the small intestines was inflamed throughout its extent. The liver seen in its natural position was more than twice the normal size; its surface was covered with innumerable brick-dust colored spots, indicating a high grade of inflammation; when taken out and cut into, the whole structure was found to be decomposed, of a dingy yellow color, and spongy in texture. The gall bladder was at least five or six times its natural size, of a bright yellow color, and filled with over a pint of dark fluid bile. The spleen or milt was ten times larger than usual, its surface mottled with yellowish or brick-dust colored spots; its structure utterly decomposed and filled with black grumous blood, rendering it difficult to handle without falling to pieces. The kidneys were both very much enlarged and diseased; the left was in a state of partial disorganization, its secreting structure being filled with dark blood. The urinary bladder was enormously distended with at least $1\frac{1}{2}$ gallons of dark bloody urine.

With the vital organs as much diseased as was revealed by this examination, and the rapid course of the disease to a fatal termination, it is manifest that treatment of any kind would have been unavailing. The disease is unquestionably "*splenic fever*"—known to have existed during the summer months in lower tidewater counties from an early period of this century, during the extremes of dry or wet seasons, when cattle are moved about from place to place, especially from the south side of the James River northwards, and from low grounds to high lands, and *vice versa*. The disease has generally been known as bloody murrain, from the constant attendant symptom of bloody urine. Calves are said to be partially exempt from the disease, and young cattle sometimes recover, but milch cows and working oxen rarely survive an attack, this terminating fatally in the course of a week. The *post mortem* conditions are almost identically the same as those seen in animals affected with disease in the lower country before the war, and long before Texas cattle were brought into the State.

In regard to the character of the disease as it appears in Virginia, much will be found in other sections of this report. The peculiar features of Texas fever are seen in all the descriptions, from Patrick County in the southwest to Hanover on the northeast. That is, the inhabitants have learned from long experience that it is dangerous to import any animals from the North or West with the exception of calves; they suffer more particularly from the loss of milch cows; cattle on enclosed pastures which have never been infected are seen to be safe; the roads and commons are everywhere the great source of danger; apparently healthy cattle from counties south or east are connected with the dissemination of the contagion; the disease is everywhere characterized by the drooping head, lopped ears, staggering gait, and bloody urine. It is impossible for us to ignore the plain deductions from these facts.

The great agricultural writers of Virginia, who have molded public opinion for the past half century, have been almost unanimous in looking upon the "distemper" of this State as being simply a malarial or "bilious" affection, induced by the peculiar climatic conditions of the tidewater counties of Virginia and North Carolina. In this they do not differ greatly from the inhabitants of all the States south of them, even including the Gulf coast and Texas. But when a disease, whether of a "bilious" nature or not, may be carried for hundreds of miles from these malarial sections, when we see the infected territory gradually extending itself away from the tidewater; over hills and mountains into regions free from the so-called malarial and bilious diseases of people, causing greater destruction and showing greater virulence than our most dreaded plagues, it certainly deserves attention. Practically, it matters little whether we call it Texas or Spanish fever, distemper or murrain; whether our philosophical conceptions of it incline us to class it with the contagious fevers, or with the climatic, malarial, or "bilious" affections, the facts remain—we cannot shut our eyes to them—and any theoretical discussions which are intended to hide any of these facts, must be looked upon as mischievous and dangerous in the extreme.

The disease is carried to the most healthful parts of the country, killing nine-tenths of the exposed cattle; the territory where this infection originally occurred spontaneously has increased by thousands and tens of thousands of square miles, and is continually advancing. These are facts which no amount of theorizing can affect, and sooner than neglect or ignore them, as has been done in the past, by the indolent philosophers, who prefer to give a week to idle speculation rather than an hour to active investigation, we had better forget that this disease came from a malarial section and that there are such names as Texas fever, distemper, and murrain. It has been the great object of this investigation to learn the facts, and, when these are once obtained, it is believed that there will be no trouble in forming theories to explain them.

To sum up, then, I believe it has been demonstrated beyond any possibility of successful contradiction: 1. That the district permanently infected with Texas fever is being continually enlarged by the advance of the infection towards the north. 2. That this infection has already reached in a part of Virginia to about 38° of north latitude. 3. That districts have been permanently infected for years not only at the line of frost and snow but where the temperature frequently descends to the neighborhood of zero, Fahrenheit. 4. That frost, snow, or even intense freezing cannot be depended upon to destroy the germs of this disease. 5. That while large rivers and mountain chains have checked the advance for a considerable time in the past, rivers at least have always been crossed in time, and there is, consequently, no apparent natural obstacle to the continued extension of the infected district towards the north.

THE LOSSES IN VIRGINIA FROM TEXAS FEVER.

It was so clearly impossible for me, with the small number of assistants at my command, to gather statistics of all the losses in a great State like Virginia, that I gave instructions to follow the line of infection, and, while tracing this and investigating individual cases, to obtain the number and value of all animals that were reported as lost. Since this was an object of secondary importance, and because the demonstration of the line of infection required more time than we could give to it, it followed, as a consequence, that no one went out of his way to collect statistics of losses. Those that are reported, therefore, represent but a small portion of the destruction even during the past summer; they are probably not one-fourth of what occurred in many of the sections which are mentioned, while equal, and in some cases, greater losses happened in many sections from which we have no reports whatever.

The following table is a condensed summary of the cases occurring during 1882, which it was necessary for us to investigate to establish the line of the infected district:

County.	Number of deaths.	Value.
Patrick.....	22	\$681
Henry.....	32	675
Pittsylvania.....	67	1,039
Franklin.....	61	1,180
Campbell.....	126	2,596
Charlotte.....	75	1,450
Buckingham.....	40	876
Albemarle.....	47	1,191
Goochland.....	10	310
Bedford.....	18	490
Tazewell.....	20	400
Fauquier.....	18	360

Number of deaths investigated, 536. Value of these animals, \$11,198.

The losses in Halifax, Chesterfield and Henrico we know to have been large, but we have no reports specifying individual losses and values. Besides the cases included in the above, we have investigated outbreaks which have occurred in former years, and for the most part quite recently, in which 454 animals have died, which were valued at over \$20,000.

The losses in this State, then, are annual and very heavy; so heavy, indeed, as to nearly arrest the development of the live-stock industry, which but for this disease would prove one of the best and surest sources of profit at the farmer's command. With the greater demand for cattle, and the buying of a larger number within the infected district, such losses must necessarily increase from year to year, unless some legislation can be devised and enforced which will limit or prevent the encroachment of such cattle during those months when the transportation of the active germs occurs. It is probable that not less than \$200,000 worth of cattle die annually from this disease in Virginia,

while in seasons of peculiar virulence, which frequently happen, several times this sum would not cover the ravages. In the counties now being infected a larger number of cattle are raised, and these are of better quality than in the counties which have generally suffered in the past, and for this reason, if the disease is left to itself, we must expect the total loss to the State will steadily increase from year to year.

ESSENTIAL NATURE OF TEXAS FEVER.

Among all the communicable diseases affecting our domesticated animals, it would be hard to find a single one the peculiar characters of which make it so difficult to investigate as Texas fever. In the first place the period of the year during which it occurs is extremely limited, being as a rule practically confined to August and September. There are, of course, many cases in July and October, but they are generally isolated, and before the investigator reaches the locality the animals are dead and too much decomposed to be useful for his purpose. Again, the period of the disease is so short and the plague so fatal that it runs through a herd and often destroys the susceptible animals, as in the case just mentioned, before the investigator can reach the ground and avail himself of the opportunity.

The fact that apparently well animals disseminate the contagion and that sick ones as a rule do not, is so completely at variance with what occurs with those diseases which have so far been successfully investigated that we have nothing in science to which we can turn with any hope of assistance. The investigator is thrown entirely upon his own resources, and, but for the fact that the instruments of to-day have reached a high degree of perfection, he would have little advantage, even now, over the physician who encountered similar problems a thousand years ago. And, when we consider that the instruments referred to cannot be used outside of a complete laboratory, and that they have for the most part been dispensed with in this investigation for want of such a laboratory, it will be understood that we have occupied very much the same position as the physician and veterinarian who have tried to grapple with these plagues during all the past history of the professions, and have continually and signally failed. If it had not been for our private laboratory, where our microscopical apparatus and reagents could be used to advantage, and where we had fitted up an incubator, imperfect for many purposes, it is true, but still one in which cultivations could be made very successfully, the scientific investigation for the year with this disease must of necessity have been a failure, as such investigations have so generally been in the past.

Finally, the success of investigators with other diseases has been due to the fact that they were inoculable maladies, that there was a known and sure method of producing them at pleasure, and that certain liquids of the body had been demonstrated to contain the germs even if the nature of these had not been discovered. With Texas fever, on the

other hand, no one knew how to produce it for experimental purposes; no one knew that it could be inoculated, and we were assured by those who had investigated it before us that no part of the liquids or organs contained a virus, and that the plague could not be inoculated. Truly these were obstacles to contend with such as no successful investigator has encountered before. In spite of this, however, it will be seen that a substantial advance has been effected—one from which it will be possible to continue the investigations with much greater chance of success in the future.

The blood is free from parasites.—In his report to the Metropolitan Board of Health, of the microscopical studies of the solids and fluids of animals affected with Texas fever, Dr. R. C. Stiles says:

Quite early in this investigation my attention was attracted to the existence in the diseased bile of minute vegetable germs, which multiplied abundantly in the various specimens of bile preserved for analysis. They existed in the form of spherical or irregular aggregations of micrococcus, the nature of which could be determined only by the employment of the highest powers of the microscope, and by studying their development. They were found in fresh blood and bile, but with difficulty. In specimens of bile collected in the evening they would be found abundantly in the morning; the white color of their aggregations contrasting with the yellow hue of the flocculi of the bile to which they were attached, and from which they seemed to be derived, their abundance being such as to preclude the idea of their derivation from any other source than the blood or the bile itself. Within a few hours of removal from the body numerous cryptococcus (or torula) cells, resulting from the development of the former, were found, often containing crimson granules.*

Billings and Curtis also concluded that in the blood, bile, and urine of cattle slaughtered in Texas, apparently healthy while alive, but presenting after death the appearances considered characteristic of the splenic fever, there are present minute bodies, corresponding to the micrococcus of Hallier, which exhibit the same behavior with reagents as the spores of fungi.†

Of course, in my search for the disease germs, it has been a primary object with me to determine if the blood of affected animals really contains either bacteria or the spores of any kinds of fungi. The blood for examination has always been obtained with the greatest precautions, vacuum tubes being filled directly from the veins and immediately sealed. When examined, after intervals of from a few hours to several days, such blood is generally found to contain granules resembling micrococci in certain respects. Many would decide at once that they were such organisms. The cultivation apparatus is a much better test for the nature of such bodies than the microscope, and, therefore, I have relied upon this. In none of the specimens of blood which I have tested in this way, though these were obtained from the jugular, the heart, the portal vein and the splenic vein, have I ever been able to discover any living germs. And, when I add that of a number of in-

* Report of New York State Cattle Commissioners in connection with the Report of the Metropolitan Board of Health in relation to the Texas Cattle Disease, p. 131.

† Diseases of Cattle in the United States. Department of Agriculture, 1871, p. 156.

oculations made with blood injected hypodermically no results have been produced, I think we may conclude that, as a rule, the blood contains no living germs in this disease, and that if the plague is due to a parasite this does not multiply in the blood before death.

The parasites of the bile.—Specimens of bile, obtained with equal precautions, give very different results. This liquid contains the most diverse organisms, sometimes schizophytes, and sometimes chains of higher fungi. Even in freshly gathered bile, these are easily discovered, and frequently all doubt of their nature is set at rest by the activity of their movements. In bile from animals affected with Texas fever, the diplococcus, which I shall soon describe as existing in the spleen, may be seen floating about in company with bacilli and bacteria. Cultivations of bile, however, have nearly always given an apparently pure growth of *Bacterium termo*. This is evidently due to the great activity of these bacteria, where the conditions are favorable to their multiplication; they are added to the cultivation liquid in equal or greater number than the other germs, and at once crowd them out and appropriate the oxygen and nutriment to themselves.

The bile, then, is not a suitable liquid in which to look for pathogenic germs; it is easy to find bacteria in it, but they may or may not be connected with the causation of the disease, and there is no possible method at our command for deciding this point.

If Drs. Stiles and Billings and Curtis found micrococci in the blood and bile, then, the discovery throws no light on the pathology of the disease; for those found in the blood were either granules of *débris* or atmospheric bacteria, and those in the bile were in no way differentiated from the numerous septic forms which are always present in that liquid.

Micrococci of the spleen and liver.—These are the organs which present the most decided lesions in this disease, and they are the ones to which we naturally turn in our search for a *contagium vivum*. My first investigations of these organs were made by dropping small pieces in alcohol, these having been removed as soon as possible from an animal recently dead of the disease. After such specimens were hardened sufficiently, thin sections were cut, stained with aniline violet and mounted in balsam after the process of Koch. Sections prepared in this way plainly showed granules of the dumb-bell or figure 8 form, which were stained a different shade from any part of the tissue. I did not consider, however, that this was sufficient proof of their having been living germs; for if we rely upon one variety of test in such investigations, we can scarcely fail to commit such egregious blunders as for a long time characterized the greater part of the investigations of these diseases.

As a more secure test of the nature of these granules, I resolved to attempt cultivations of them; but this proved to be a more difficult matter than had been anticipated. The cases of the disease always occurred at a long way from the laboratory, and how to convey pieces of spleen a distance that required several hours, or even a day, to travel,

without their becoming contaminated with atmospheric germs, was a problem of no small magnitude. Fortunately the spleen in this disease very frequently becomes disorganized, and the contents assume a semi-fluid consistency. It was found that, in these cases, vacuum tubes might be successfully filled with splenic pulp, though it was only with the greatest difficulty that they could be sealed, as the thick pulp formed a coating which prevented the cohesion of the glass.

August 1, 1882, I was successful in filling and sealing a number of tubes from the spleen of a cow which had just died in Bedford County, Virginia. The *post-mortem* appearances noted in this case were as follows: Animal not yet cold; fat slightly colored with yellow; petechiæ and large discolorations on external wall of heart, also on inner surface of the left ventricle near the apex, and on inner surface of right auricle; the third stomach impacted with dry food; mucous membrane of fourth stomach much congested, and with many erosions and ulcerous sores; liver engorged, gall-bladder much distended; spleen greatly enlarged, very dark and partially disorganized; bloody discoloration of tissues around the kidneys, and engorgement of these organs; bladder distended with claret-colored urine.

August 7, one of these vacuum tubes was examined microscopically. No decomposition whatever had occurred. Besides the *débris* of the spleen, there were blood corpuscles of perfect form and granular cells or lymph corpuscles. Then there was a large number of spherical granules united by twos, or, in other words, dumb-bell micrococci, or diplococci, as we choose to call them; also, some rod like bodies not very numerous and suspected, from their appearance, to be coagulated fibrin. In the specimens stained with aniline violet, the rods were invisible but the granules, or diplococci, were very plain.

Two cultivation tubes were infected with some of this pulp, as soon as the vacuum tube was opened. The next day both were turbid and something had evidently multiplied in them. One was examined and found to be a pure cultivation of diplococci, without any power of movement; they resembled fowl cholera micrococci, but were smaller. The second was an impure cultivation, containing diplococci in abundance, but also bacilli.

August 8, my assistant, Mr. W. B. Shaw, slaughtered an animal that had been sick several days, and sent me vacuum tubes filled from the spleen, the gall-bladder, the splenic artery and the portal vein. The lesions in this case were sufficiently characteristic of the disease, viz., enlarged and softened spleen; enlarged liver; distended gall-bladder, blood extravasations on spleen and heart; ulcerations about pyloric extremity of stomach, and discolored urine.

The tubes were received the 10th of August. Two, containing splenic fluids, were in excellent condition, the coagulum being firm, and the serum light-yellow and not at all stained with the red coloring matter of the blood. Both contained the diplococci found in the former case.

The tube containing bile had a plain odor of putrefaction, and the liquid swarmed with bacilli, vibrios, bacteria, and a few diplococci. Cultivations of the splenic liquids developed only diplococci exactly similar to those obtained from the former case, but having rather more of a tendency to adhere and form short chains. Blood from splenic artery, placed in a cultivation apparatus, proved to be entirely free from living germs. The diplococci were carried through four cultivations, without any change in their appearance or manner of growth.

Owing to the difficulty of obtaining suitable cases, and of sealing and transporting the vacuum tubes, these were the only reliable investigations of the splenic liquids; but they are sufficient to prove that a parasitic schizophyte multiplies in the spleen in cases of this disease. I have no desire to exaggerate the importance of this discovery; it undoubtedly needs confirmation, but this, I feel assured, will not be lacking if the investigation is continued through another summer.

Inoculations with splenic pulp.—In 1880, two inoculations were made with splenic pulp, one of which produced a very severe case of the disease, the second, a much milder attack, though accompanied with great elevation of temperature. These experiments were detailed in my last report; and, while they indicated the inoculability of the disease, they needed confirmation. To this end the following experiments were made:

Experiment No. 1.—The splenic pulp, remaining in the vacuum tube August 7, after the cultivations were infected and the microscopic preparations made, was diluted with 2^{cc} of water and injected, in equal quantities, under the skin of a steer and heifer, each being about two and one-half years old. These animals, for more perfect observation, were kept stabled. Neither showed any ill effects whatever from the inoculation.

As many other experiments on stabled animals failed during the summer, it occurred to me that, perhaps, keeping the animals stabled and out of the sun had something to do with this result. I knew that there was a general impression that stabled animals were less liable to be attacked spontaneously, but I had partly concluded that this was due to their not being exposed to the contagion, since I had investigated a number of cases that had occurred with stabled cattle farther south. On more mature reflection, however, it seemed that stabling might have a most decided influence on the appearance of the disease, in a cool climate, such as we have in this mountain section. The disease only appears spontaneously in the hottest weather, and is checked by a few cool days, to commence again with the return of the heat. Then, animals which are overheated by driving in the sun have frequently developed the disease when they probably would not have been affected if allowed to remain quiet. These reflections decided me to make some more inoculations of cattle in the open fields.

The 3d of October I obtained some fresh material by killing a cow

in the last stages of the disease, at Bellevue, Va. Her temperature before death was 105°. The spleen was much enlarged and softened. Other organs were not closely examined, as we just had time to obtain the splenic pulp and get a train that would take us to where our cattle were grazing. The symptoms, however, were so plain as to leave no doubt of the disease.

Experiment No. 2.—The afternoon of October 3 we inoculated, by hypodermic injection, in the side of the neck, a large white cow, with the splenic pulp obtained in the morning. This cow was in excellent condition, and was selected on that account as being, probably, more susceptible. A small heifer was also inoculated in the same manner, and a steer, about two years old, was drenched with about two drachms of the pulp that was left from the above inoculations. These animals were in a field with ten or twelve others. I was unable to watch, personally, the effect of this experiment, but my assistant, Mr. Shaw, reported to me that the cow was taken sick October 13. The following day the symptoms were very intense; she was lying with head stretched out and ears drooping, and appeared to have high fever. When made to walk, the gait was staggering, there was knuckling at the fetlocks, drooping head, and lopped ears. She was not observed when voiding urine, but near where she was lying was the evident appearance that this had lately been done, and the remains upon the grass indicated that this liquid had been highly charged with blood. This animal died during the night of October 16, the neck and shoulder from the point of inoculation having become considerably swollen. The young animals seemed ailing for a day or two, standing by themselves and appearing dull, but in neither case was there any serious sickness.

In this experiment we have not only a very evident confirmation of my former investigations, but it is proved to us that young animals cannot be relied upon to give results. The disease is, then, inoculable, and the splenic pulp is the material which contains the virus. Out of five inoculations, with this material, we have produced three plain cases of the disease. Surely, then, it can no longer be maintained that this is not an inoculable fever.

Inoculations with cultivated micrococci from splenic pulp.—The discovery and cultivation of a schizophyte, in a state of purity, is no proof of its pathogenic action; the disease must be produced by inoculations with the organism that has been cultivated in a liquid known to be harmless. Having discovered that the splenic pulp of diseased animals acts as a virus when inoculated upon susceptible subjects, and that this pulp contains, apparently, but one organism, the next step in our investigation was to test the effect of inoculations with the cultivated parasite. Accordingly, the following inoculation experiments were made with the cultivated micrococci:

Experiments Nos. 3 to 7.—August 14, five head of cattle were inoculated by hypodermic injection of the first and second cultivation of

diplococci, in the quantities named. No. 1 received $1\frac{1}{2}^{\text{cc}}$ of first cultivation; No. 2, same quantity of second cultivation; No. 3, 2^{cc} of second cultivation; No. 4, 3^{cc} of second cultivation; No. 5, 7^{cc} of first cultivation. These inoculations produced no noticeable effect.

Experiments Nos. 8 and 9.—August 27, 20^{cc} of the second cultivation was injected under the skin of No. 4, and a like quantity of the third cultivation was administered in the same manner to No. 6. These also were without result.

Experiments Nos. 10 and 11.—September 1, No. 4 received, by hypodermic injection, 24^{cc} of a fourth cultivation, and No. 6, 15^{cc} of same liquid. This was followed by some coughing, elevation of temperature to $104\frac{1}{2}^{\circ}$, and constipation of bowels, but whether there was any connection between this result and the inoculation could not be determined. It is certain that there was no serious illness in any case.

What conclusions are we to draw from these experiments? They are certainly negative. They do not show that our diplococcus is the cause of the disease, nor do they show that it is not. These cattle were not good subjects, though they were the best that could be obtained. They were too young, not fat enough, and, besides, were stabled. I have already shown that two of them resisted inoculations with splenic pulp, which in some cases has proved extremely virulent. Again, the pulp had been inclosed for several days in vacuum tubes, and the cultivations were allowed to stand for some days in contact with the air before they could be used. Pasteur's recent investigations show us what disastrous effects such treatment may have on certain kinds of virus. It seems to me, therefore, that this micro-organism must be studied more carefully before we can reach anything like definite conclusions in regard to it. The fact that the splenic pulp causes the disease, and that this appears to be the only germ which is contained in it, is certainly very strong evidence of its connection with the virus. Such failures in experiments are what must always be expected in investigations of difficult questions, and, while they are somewhat discouraging, they at least give us new ideas in regard to the future direction of our work.

THE DISSEMINATION OF TEXAS FEVER.

We know that an animal in apparent health, coming from the district permanently infected with Texas fever, may poison the pastures upon which it travels to such an extent as to prove fatal to a large majority of the susceptible cattle which graze upon them. How is this poison carried, and in what manner distributed? Does it exist in the saliva, as asserted by some? Is it disseminated with the droppings, as supposed by others? Or is it carried in the hair as impalpable dust? Evidently these are questions of considerable importance to us in our endeavors to devise means of preventing the continual extension of the disease. It was a part of my plan to elucidate such questions by the experiments of the past summer, but there were so many unknown difficulties to contend with that the success was not flattering.

Inoculations with excrement of Southern cattle.—One of the most reasonable suppositions, in regard to the infection of pastures, is that this occurs from the solid excrement of the southern cattle. These cattle have acquired an immunity from this disease because it is one of the non-recurrent fevers, and it is not at all probable that the germs would multiply in the body of an insusceptible animal to a sufficient extent to be distributed by the urine. The digestive cavities are practically outside of the body, however, and, from the character of their liquids and the temperature at which they are kept, they are extremely favorable for the multiplication of micro-organisms of the class to which disease germs belong. It is, therefore, highly probable that the germs of this disease taken into the alimentary canal of even insusceptible animals in large numbers would multiply for several months, and be distributed during all this time with the solid excrements.

To test this theory cattle excrement was taken from the Savannah stock-yards in May, placed in tin cans, and used for inoculating purposes within three days. The following are the details of the experiments:

Experiments Nos. 12 to 15.—Fresh cattle excrement was taken, May 20, at the Savannah stock-yards and sealed in tin cans. May 23 a part of the droppings from cattle that had been shipped from South Carolina just opposite Savannah was mixed with that from cattle that came from Coffee County, Georgia. The whole was rubbed in a mortar with a three-fourths per cent. salt solution until a thin paste was formed. This was filtered through two filters of fine cotton cloth to remove the coarser particles. The filtrate contained a variety of bacterial forms, a large majority being of the dumb-bell shape. Four cattle received hypodermic injections of this filtrate in the dose of one drachm. To one of the animals, three ounces were administered by the mouth in addition to the hypodermic injection. The animals were carefully examined and the temperature taken daily, but there was no marked disturbance of the general health. At the point of inoculation in each case there was very considerable swelling and tenderness which terminated in an abscess. The pus of these abscesses swarmed with an organism which is probably identical with the one described by Pasteur as a *pus generator*.

Here, again, our result is not at all satisfactory, because if the germs of Texas fever were present in the excrement they may have been overcome by the pyæmic germs, or the weather may have been too cool, or the cattle not susceptible enough for the development of the plague to occur. There are so many elements of uncertainty in regard to this disease that the whole subject requires a thorough experimental investigation before any safe conclusions can be reached.

Is the disease spread by other means than infected pastures?—Those who have given much time to the investigation of Texas fever, in the past, have, so far as I am aware, been unanimous in the conclusion that the

disease is only contracted from infected grounds. In other words, that the germs are not disseminated through the air, or conveyed in any manner directly from one animal to another. Within the last year or two different views have been advanced, and, in some cases, by members of the veterinary profession. Generally these have been based upon theoretical considerations and reasoning from what is seen in other affections to what we ought to see in this one. In one or two instances the superficial observation of a few cases has seemed to support the opinion that the disease might be contracted directly from sick animals, or even that the infection might be carried a considerable distance through the air. It has also been stated that calves contracted the disease from their mothers. A brief review of the observations bearing upon this point is, therefore, advisable at the present time, when the interest in regard to the possible means of preventing the disease has become so great.

Turning to the report of the New York State Cattle Commissioners in connection with the report of the Metropolitan Board of Health, 1868, we find that Dr. Manheimer, who investigated the many cases occurring in the Fifth Ward of Chicago, stated :

It seems that native cattle do not communicate the disease to each other, as in many instances cows were housed in the same stable with sick cows without being infected. * * * In a circuit of about two miles only one cow escaped the disease, and that one was kept in the stable for the last three weeks (p. 82).

Mr. Hosack, cattle inspector of Pittsburgh, Pa., reported as follows in regard to cases at that point :

The only cattle that were affected were the two droves from Illinois. * * * All the cattle on that train were more or less affected. After their arrival they were placed in the stock-yards near other cattle, in fact I can say they were surrounded by other stock, but in no one instance did the disease show itself save in the two herds above spoken of (p. 84).

E. C. Hall, clerk of the board of trustees of Onarga, Ill., wrote :

At Loda, a railroad station about 16 miles south of here, there was a load of Texas cattle unloaded from the cars and driven east across the country to Indiana, which I believe is the northern limit where the disease has been. The native cattle there having communication with the Texas cattle, or herded upon their track or herding grounds, have become diseased, and it has been very fatal, sweeping off nearly whole herds. The cattle in pasture along the track have not been affected. One farmer who had his stock a part in pasture and a part on herding grounds (open prairie)—those herded, many of them died ; he put the two herds together in pasture, and not a case occurred among those that had been pastured (p. 85).

Fred. A. Atkins, of Odell, Ill., wrote :

In conclusion, I would say that I have no reason to believe that native cattle, even under circumstances the most favorable for infection, will infect other native cattle. Not one of the many cases I have seen die of this disease but that was exposed to infection from Texas stock ; and not one of those in this vicinity now living but that was exposed to sick native cattle. I have seen a calf, which is now living and in good health, that was suckled in succession by three different cows which died of this disease in its most aggravated form. The little animal drew its food from them while they were sick, and when the first died it was given to another, and so on, and the process had no deleterious effect upon its health (p. 86).

Dr. Thomas L. Neal, health officer of Dayton, Ohio, reported :

No instance occurred where a native contracted the disease which had not been exposed directly to the ground where the "long horns" had grazed (p. 87).

Dr. J. F. Hodgen, president of the Saint Louis board of health, made the following statement :

Cattle running about the stock-yards where Texas cattle are received are affected, while others near by, but kept up closely, are not diseased (p. 88).

Dr. James W. Clements, late health officer of Saint Louis, wrote :

I met with or heard of no cases among cows kept confined either to stables or their own pastures except one instance. A large dairyman, in June, purchased some forty head of cows in Illinois. Shortly after arriving at his farm, a disease appeared among them from which they died rapidly. The disease did not show itself among his old stock; this may have been due to the segregation of the Illinois or sick ones (p. 89).

The instance referred to here is no exception to the rule, as was supposed by the writer. The purchased cows had been driven over infected grounds on their way to their new home, and contracted the disease in this way. The fact that the home stock was not affected is another strong confirmation of the rule which has been so long accepted as true.

V. P. Chilton, of Southwestern Missouri, stated :

I have had my own cattle separated from large herds of Texas cattle by a fence without any evil results, and of the immense number that have died on this road none have died on pastures from which Texas cattle have been excluded. The instances to sustain this view are so numerous that I will not undertake to give them (p. 93).

Professor Gamgee, after a very extended trip through the affected districts, came to the conclusion that there was not a single instance to show that sick northern animals had induced any disease, either directly or indirectly. (Report, p. 115.)

Dr. W. S. Morton, of Cumberland County, Virginia, published a treatise on the Distemper of Cattle, in June, 1854, in which he stated :

For many years I kept two herds on the same farm—one infected, the other sound. A man with a *white skin* was sent to pull and leave down the barrier between them. Unfortunately, before matters could be rectified, a sound ox walked, grazing, for nearly 50 yards on the infected grass. He died of distemper in a few days. I kept those two herds separate from the last of June until nearly Christmas.*

In the outbreak on the farm of Henry Preston, at Wallace's Switch, Va., in 1878, it was noticed that the calves never showed the least evidence of disease, though suckled by their dams up to the time of death. At the same farm, in 1868, three of a herd of 65 were infected and died without the disease extending to the rest of the animals.

During my experiments in 1881, two cattle contracted the disease from inoculation, and though confined in a rather large yard with six others, none of the latter were affected with the plague.

The three animals which were last October infected with splenic pulp of a diseased cow were grazing on a pasture with about a dozen others,

* Quoted from report of Dr. J. M. Hines.

and though one of the infected ones died and the other two were sick, the remainder of the herd retained the best of health.

From all parts of the line examined during the summer and fall, my assistants reported to me that the farmers were able to preserve their cattle, even when surrounded by permanently infected commons, if they kept them securely fenced on pastures to which the disease had never been carried.

We have, then, a mass of independent testimony, coming from all parts of the country, which is practically unanimous in asserting that susceptible cattle do not contract Texas fever either from southern cattle or from sick natives. I have so far seen no reason to doubt that this conclusion is correct, and when I am told that in a Northern State, during the past summer, Texas fever was contracted at a distance of 150 yards from any source of infection, and that sucking calves contracted the disease from the cows, I am inclined to believe either that the facts were not carefully observed or that there was a mistaken diagnosis, and that the disease was not Texas fever.

Are pastures ever infected by sick natives?—If the observers of Texas fever are practically unanimous in concluding that the disease is never conveyed directly from one animal to another, this is far from being the case in regard to the ability of sick animals to infect pastures. It is true that in all the observations of 1863 there were but two cases where it seemed at all certain that pastures had been poisoned by sick northern cattle. In regard to these, however, there was little chance for doubt. In my own investigations, I have generally found that sick natives were harmless, but there seem to be occasional instances, particularly where they have pastured on permanently infected lands, in which they carry the poison and infect lands that were previously safe. I give below a few examples of such supposed infection as reported by my assistants:

Dr. Watkins, of Martinsville, Henry County, Virginia, bought 16 head of cattle 10 miles east of Appomattox Court-House, in February, 1875. There had been no infection on his place for forty years. A steer, belonging to a negro on the farm, got out of the pasture and went up the river where the disease was at the time. He was brought back and put in with the other cattle, but died soon after. The Appomattox cattle all contracted the disease and died, though this was three years after they were brought to the farm.

Dr. Stovall, of the same place, had two milch cows on his farm, where there had been no disease for twenty years. Two yearlings, which had sickened on the neighboring common, were placed with the cows and died in a few days. Both cows contracted the disease and died.

B. S. Jones, of Ridgeway, Virginia, took a cow from the common and placed her on the same pasture with other cattle. She had been infected on the common, contracted the disease, and died. Two of the other cattle afterwards sickened and died.

V. Brooks, who lives not far from New Canton, Buckingham County, Virginia, drove a yoke of oxen to New Canton and back home in 1879. They contracted the disease from infected roads, and before dying infected his home pastures, causing a loss of seven other cattle.

It would seem from such facts that sick cattle may occasionally infect pastures, especially if they have contracted the disease on permanently infected lands. But these instances are so few, compared to the cases where, under exactly the same circumstances, they have been perfectly harmless, that we are almost tempted to believe that the infection in these cases might not have been correctly traced. Still, there seems to be no apparent reason why a susceptible animal may not carry the germs from a permanently infected pasture as well as an insusceptible one. The reason that, in the Northern States, the sick cattle do not disseminate the contagion may be because the pastures on which they have grazed were not so thoroughly saturated with the disease germs as are the permanently infected lands of the South. It seems best, therefore, for us to keep these facts in mind, and, until we understand more of the germs and of their habits of life and the way in which they are distributed, to consider that, under certain circumstances, sick animals may be a source of danger.

Rate of advance of the infected district.—In collecting other facts in regard to Texas fever, I have endeavored to obtain data which would enable us to determine the rapidity with which it is advancing, and the time which it will require in the future to gain a certain distance. The evidence bearing upon this point is still very insufficient, but it is not without considerable value; it certainly gives us a more definite idea of the matter than we have ever had before. The most rapid progress, for a long series of years, has occurred in the State of North Carolina, in the extension of the disease across the State from east to west.

About fifty years ago, as would appear from laws enacted at that time, the border line of the infected district was somewhere east of Raleigh, where the character of the timber changes and the long-leaved pine appears. This line is now at the Blue Ridge Mountains, a distance of at least two hundred miles, or an average of four miles a year. There is no other example of such rapid progress, probably because, the same parallel of latitude being followed, there was less change of climatic conditions than is encountered in going the same distance toward the north.

In Habersham County, Georgia, from information which I obtained during a visit to that section in 1881, there appears to have been an advance of about twenty miles in the course of ten years, or two miles a year. As this progress was made through the foot-hills of the Blue Ridge, to a considerable extent, the conditions must be regarded as unfavorable for the extension of the infection.

In Franklin County, Virginia, it has advanced westward twelve miles in three years, or four miles a year, a rate equal to that in North Carolina

but for a much shorter time. In the neighborhood of Burnt Chimneys and Leatherwood, in Henry County, Virginia, the progress has been rather over three miles a year for the last two or three years. The rate of progress in various parts of Pittsylvania County, as near as I was able to ascertain, was about fifteen miles in ten years, or one and a half miles per annum. In Halifax County it does not seem to have advanced more than twenty miles in sixty years, or one-third of a mile per year; in Campbell County the advance has been but ten miles in fifty years, or one-fifth of a mile a year; while in Buckingham County there seems to have been scarcely any new territory covered for the last fifty years.

The progress of the disease cannot be considered at all regular, therefore, in some places it may reach four miles per year, while in others it may be five years in gaining a mile; but looking at the infected district as a whole, there has been, undeniably, a continual advance at a comparatively rapid rate.

Necessity for a closer study of the virus.—There are many problems, connected with this disease, which are still mysteries, but which it is extremely desirable should be cleared up. We are not yet certain as to the germ which is responsible for the trouble, and much less do we know anything of its habits of life, and the means of destroying it. There is some reason why animals though exposed to the infection do not, as a rule, sicken until after the middle of July; this may be owing to the higher average temperature, but we are by no means certain.

The germs, at times, seem to be extremely sensitive to frost and cold weather, but in spite of this they somehow resist winters when the temperature sinks to zero. Is this because they descend in the soil so far that they are not exposed to this low temperature? Or is their apparent destruction by frost a mistake, and the cessation of the disease due entirely to the lower average temperature of the late fall, or to the fact that, the vegetation being killed by frost, the cattle no longer gather them up in sufficient quantity with their food?

There seems to be no doubt that this is a non-recurrent fever. Is it possible for us to learn enough of the conditions necessary for its production, and to so attenuate the virus that cattle exposed to it may be protected from its ravages by vaccination?

Finally, there are facts which indicate that pastures, even though permanently infected, if left to themselves, may be made safe in the future for the most susceptible animals; in other words, that by proper treatment the worst infected lands of the South may be freed from this virus. Is this true? Can the disease be not only checked in its advance but eradicated from the lands over which it has spread itself?

It may take years to solve these questions, but of what vast importance are they, not only to the country now infected, but to that which is becoming year by year more endangered. The first step towards this solution is evidently a thorough study of the virus; the determination

if the diplococci which I have discovered in this virus are the essential agents of the disease, and, if so, a searching inquiry into their life history. Once make sure of the disease germ and the conditions under which the disease may be produced with certainty for experimental purposes, and the greatest obstacles to the investigation of this plague will be overcome.

Importance of determining the inoculability of the disease.—It has always been inconceivable to me how a disease could be disseminated by a something that could be transported hundreds of miles by animals and still be one that could not be produced by inoculation. In other words, if the disease is produced spontaneously by animals taking something into their bodies with their food or with the inspired air, there is no apparent reason why it should not be produced experimentally by introducing the same essential cause by means of inoculation. And yet our best authorities had concluded that this disease could not be produced by inoculation.

Our ability to produce a disease experimentally is one of the first requisites to its successful study. It would have been impossible to demonstrate that charbon was caused by the *Bacillus anthracis*, or that fowl cholera was caused by a diplococcus, if the artificial introduction of these organisms into the bodies of susceptible animals had not produced these diseases. It would have been equally impossible to have discovered many other facts that have been brought out during the last three years, and which have done so much to clear up the mysteries connected with the contagious fevers.

And, so, if the inoculability of Texas fever had not been discovered and demonstrated during the last two summers, we should still be in the greatest doubt as to the possibility of determining the connection of the diplococcus of the spleen with the etiology of this plague. At present, however, it seems as though this would not be so very difficult. The disease may be produced by inoculation with splenic pulp; this is one point gained, and a most important one. The fresh splenic pulp contains a diplococcus and apparently no other organism; this is fact number two, and, for our purpose, scarcely less important than the other. It is true the cultivated diplococcus failed to produce the disease when inoculated in our experiments of last fall; but, as I have already shown, this may be explained by the unfavorable conditions under which the culture was made and used.

We have some established facts to work from in the future, and I shall be very much surprised if we are not able to prove that the diplococcus of the spleen is the true germ of the disease. When this is done, we can easily study its life history and the conditions necessary for its existence. We can change it into a vaccine if desirable, we can test the effect of disinfectants upon its development and existence, and, indeed, there are few points, connected with the disease, that cannot be discovered by a careful study of its exciting cause. What has been

accomplished, therefore, must be looked upon as having an important bearing on the future investigations of this disease, and it may enable us, if our views in regard to this germ are well founded, to make as thorough a study of this plague as has been made of any other.

INVESTIGATIONS OF FOWL CHOLERA.

As there were a number of very important questions connected with fowl cholera which we were unable to dispose of in our former reports, for lack of experimental data, we have continued our investigations of this disease, and carried them as far as our time and facilities would permit.

Susceptibility of the offspring of insusceptible fowls.—There were many reasons why it seemed possible that the offspring of insusceptible fowls might inherit, if not complete immunity from the disease, at least a certain degree of insusceptibility, which would be indicated by the effects of diluted virus. Pasteur had made some experiments upon this question, and concluded that no degree of immunity was inherited, though, in the published report of his experiments, we were left in doubt as to whether both parents were thoroughly protected by vaccination. Again, the chicks were inoculated with strong virus which could only show that they had not acquired complete immunity, without giving any indication as to lesser degrees of this.

In the spring of 1882, I put a number of thoroughly vaccinated hens and a vaccinated cock in an inclosure, where they were completely isolated from other fowls. These were, from time to time, inoculated with strong virus to make sure that their immunity was not lost during the experiment. Their eggs were hatched, and the following experiment proves the susceptibility of the offspring:

Experiment No. 1.—Two chickens, five or six weeks old, the offspring of insusceptible fowls, were inoculated June 21 by a single lancet puncture with virus diluted 1 to 2,500. June 27, one has plain though slight lesion, the second has but a faint appearance of one. June 29, both have the local congestion which demonstrates the multiplication of the virus. July 7, the lesion in each case is very much developed, though both remain well. July 13, the lesions have nearly disappeared; both in excellent health. This virus tested on other fowls at the same time, and in the same strength, produced no greater effect than on the chickens. We may conclude, therefore, that no appreciable degree of immunity can be conferred by fowls upon their offspring in this disease.

Effect of sterilized virus on susceptibility.—From the considerations which were discussed at sufficient length in my preceding report, it seemed that the immunity acquired, in regard to contagious diseases, from a first attack, must be due to the living matter of the body becoming inured to the chemical products formed by the disease germs during

their multiplication. That it is not due to any chemical changes in the composition of the tissues, is sufficiently apparent from the fact that broth made from insusceptible fowls is still a perfect cultivation liquid for fowl cholera micrococci.

If we introduce a small number of these micrococci into the tissues of the most susceptible fowl they produce no effect whatever, as is seen in experiment number 7, where fowls inoculated with virus diluted as 1 to 125,000 did not develop even a local irritation, although in this case from ten to twenty micrococci must have been placed beneath the skin. This would indicate that, in the perfectly healthy tissues, the conditions are such that the bacteria cannot multiply. When we introduce a larger number of germs, there is a correspondingly increased quantity of the chemical products not only placed in the tissues at the time of the inoculation, but continually being formed. These products we know to be poisonous from their effects on fowls when injected beneath the skin in a concentrated condition and free from living organisms.

Referring the reader to my previous report for a complete theory of insusceptibility and its production, I will simply detail the experiments that have been made up to this time to throw more light upon this difficult question.

Experiment No. 2.—Nine fowls bought in the market May 25, 1882, were divided into four lots. Lots 1, 2, and 3 are of two fowls each, and receive hypodermic injections of cultivated virus sterilized by heat, and in case of the larger doses concentrated over a water bath. Lot No. 4 contains three fowls, and is preserved without treatment for comparison. The injections were made three times daily for twenty-two days, commencing with small doses, and gradually increasing until with lot 3 they reached what represented one ounce of cultivation liquid. The following table shows the hour and quantity of each dose:

Number of dose.	Day.	Hour.	Lot 1.	Lot 2.	Lot 3.
			<i>Minims.</i>	<i>Minims.</i>	<i>Minims.</i>
1	May 27	9.30 a. m.	1	4	8
2	May 27	1.30 p. m.	2	8	16
3	May 27	7.15 p. m.	3	12	24
4	May 28	9.00 a. m.	4	16	32
5	May 28	2.30 p. m.	5	20	40
6	May 28	7.00 p. m.	6	24	48
7	May 29	6.30 a. m.	6	28	56
8	May 29	2.00 p. m.	8	32	64
9	May 29	7.30 p. m.	9	36	72
10	May 30	8.00 a. m.	10	40	80
11	May 30	1.15 p. m.	11	44	88
12	May 30	7.30 p. m.	12	48	96
13	May 31	6.45 a. m.	13	52	104
14	May 31	1.30 p. m.	14	56	112
15	May 31	7.15 p. m.	15	60	120
16	June 1	7.30 a. m.	16	64	128
17	June 1	1.30 p. m.	17	68	136
18	June 1	7.30 p. m.	18	72	144
19	June 2	7.15 a. m.	19	76	152
20	June 2	1.30 p. m.	20	80	160
21	June 2	7.30 p. m.	21	84	168
22	June 3	7.45 a. m.	22	88	176
23	June 3	1.45 p. m.	23	92	184
24	June 3	7.00 p. m.	24	96	192
25	June 4	7.00 a. m.	25	100	200
26	June 4	3.15 p. m.	26	104	208

Number of dose.	Day.	Hour.	Lot 1.	Lot 2.	Lot 3.
			<i>Minims.</i>	<i>Minims.</i>	<i>Minims.</i>
27	June 4	7.45 p. m.	27	108	216
28	June 5	7.30 a. m.	28	112	224
29	June 5	1.30 p. m.	29	116	232
30	June 5	7.30 p. m.	30	120	240
31	June 6	7.30 a. m.	31	124	248
32	June 6	1.15 p. m.	32	128	256
33	June 6	7.30 p. m.	33	132	264
34	June 7	7.30 a. m.	34	136	272
35	June 7	12.30 p. m.	35	140	280
36	June 7	7.45 p. m.	36	144	288
37	June 8	7.30 a. m.	37	148	296
38	June 8	1.30 p. m.	38	152	304
39	June 8	7.15 p. m.	39	156	312
40	June 9	7.30 a. m.	40	160	320
41	June 9	1.30 p. m.	41	164	328
42	June 9	7.30 p. m.	42	168	336
43	June 10	7.30 a. m.	43	172	344
44	June 10	1.30 p. m.	44	176	352
45	June 10	7.15 p. m.	45	180	360
46	June 11	8.00 a. m.	46	184	368
47	June 11	2.15 p. m.	47	188	376
48	June 11	7.30 p. m.	48	192	384
49	June 12	7.30 a. m.	49	196	392
50	June 12	1.30 p. m.	50	200	400
51	June 12	7.00 p. m.	51	204	408
52	June 13	7.30 a. m.	52	208	416
53	June 13	1.30 p. m.	53	212	424
54	June 13	7.30 p. m.	54	216	432
55	June 14	7.45 a. m.	55	220	440
56	June 14	12.45 p. m.	56	224	448
57	June 14	7.45 p. m.	57	228	456
58	June 15	7.30 a. m.	58	232	464
59	June 15	2.15 p. m.	59	236	472
60	June 15	7.30 p. m.	60	240	480
61	June 16	7.45 a. m.	60	240	480
62	June 16	1.45 p. m.	60	240	480
63	June 16	7.30 p. m.	60	240	480
64	June 17	7.30 a. m.	60	240	480
65	June 17	1.30 p. m.	60	240	480
66	June 17	7.50 p. m.	60	240	480

The irritation produced by the substance injected seemed to be greater with lot 1 than with the others, though one of the fowls in lot 2 was severely affected. This irritation was most intense at about the fiftieth dose.

June 21, the fowls of lots 1, 2, and 4, and one of those in lot 3, were inoculated with virus diluted 1 to 2,500; the second one, in lot 3, inoculated with a dilution of 1 to 50,000.

June 28, all the birds except No. 2, in lot 3, have marked irritation at the point of inoculation.

July 7, one in lot 2 is very sick, also one in lot 4. The bird inoculated with a dilution of 1 to 50,000 shows no results.

July 14, the sick fowl of lot 2 is dead; the others have recovered.

The birds subjected to treatment with the devitalized virus were, consequently, equally susceptible with those that received no treatment, and, therefore, under the conditions of this experiment devitalized virus is powerless to protect against the living germs.

In this experiment there was no satisfactory evidence that any immunity had been granted, and it was determined, for this reason, to repeat it with a larger number of fowls, and in case of the smaller doses to give these in full amount from the first. The duration of this experiment was two days greater than the last.

Experiment No. 3.—Twelve fowls were divided, July 7, into six lots of two each. Five of these lots received, three times daily, by hypodermic injection, doses of sterilized cultivation liquid, as follows: Lot 1, 5 minims; lot 2, 10 minims; lot 3, 15 minims; lot 4, 30 minims; lot 5 commenced with 5 minims and increased one minim each dose to one drachm, the remaining doses being of this quantity. Lot 6 received no treatment. The first dose was administered July 7, at 2.30 p. m.; the last dose July 31, at 6.30 p. m. They, consequently, received 72 doses.

August 5, each of the fowls in the above six lots were inoculated with virus diluted 1 to 1,000.

August 15, all have the local lesion produced by inoculation.

None of these birds were sick, but the effect produced on the lot that received no treatment was no greater than on the others. No immunity was, therefore, conferred. The virus used in this experiment had become somewhat attenuated by standing, or some of the fowls would have certainly died from its effects.

Here, again, although the chemical products have been injected thrice daily for a time equal to that required to produce complete insusceptibility by vaccination, and although the dose was varied so as to be almost certain to cover the quantity that would be produced by the number of germs multiplying as a consequence of vaccination, our results are completely negative. Is our theory of insusceptibility wrong, or have we failed in some of the conditions necessary to its production? We have in view some experiments which will probably decide this important question.

Vaccinations with diluted virus.—Although the experiments, detailed in the preceding report, were sufficient to demonstrate that this virus might be diluted until when inoculated it no longer produced other effects than a slight local irritation, and that this irritation was sufficient to grant an immunity from the effects of the strongest virus in the future, there was still doubt as to how far this dilution might be carried and yet produce the local irritation, without which no degree of protection resulted. There was still doubt as to the proper dilutions to use for practically producing insusceptibility with safety. To obtain more definite results on this point, a number of experiments were made which are given below.

Experiment No. 4.—Four fowls, which had been previously inoculated with a dilution of 1 to 80,000, and of which two had shown a perceptible local irritation, were inoculated, February 22, with a dilution of 1 to 10,000; March 4 the two which resisted before have plain lesions, the others are not affected.

No bad effects followed this inoculation.

Experiment No. 5.—Three birds, which had been inoculated with a dilution of 1 to 40,000 without result, were again inoculated, February 22, with a dilution of 1 to 10,000.

March 4, two have the local lesion. The third one showed no effects from the inoculation; the others recovered without signs of sickness.

Experiment No. 6.—Two birds, that had been inoculated with virus diluted 1 to 20,000 without effect, were inoculated, February 22, with virus diluted 1 to 10,000.

March 1, one has slight lesion; the other has no appearance of one. March 11, both are sick; the 12th, one died, and one day later the second one died.

Experiment No. 7.—Three susceptible Plymouth Rock hens were inoculated, February 22, with virus diluted 1 to 125,000. The inoculation produced no effect whatever.

Experiment No. 8.—The three hens of experiment 7, and a cock of the same breed, not before inoculated, were inoculated, March 28, with virus diluted 1 to 100,000.

April 3, one has the local lesion; the 8th, a second has this lesion. The others were not affected.

Experiment No. 9.—The four fowls of experiment 8 were inoculated, April 24, with a dilution of 1 to 80,000. This produced no effect.

It was not possible to carry these experiments any further during the past year. The following table is a recapitulation of the results of all the inoculations with dilutions of the standard virus (see report for 1881). In nearly all the cases where the local lesion was produced the fowls were afterwards either tested with strong virus or were exposed to infected runs, and none have died from either of these tests. The figures in the column headed "Dilution" represent the number of parts of salt solution to each part of standard virus:

Dilution.	Number inoculated.	Local lesion.	Mild attack.	Death.	No effect.
1 to 50	1	1
500	1	1
1,000	16	12	4
2,500	15	11	4
5,000	3	2	1
10,000	25	16	2	6	1
20,000	4	2	2
40,000	4	1	3
50,000	1	1
80,000	6	2	4
100,000	4	2	2
125,000	3	3
Totals ...	83	47	2	18	16

Taking these experiments, then, just as they have been made, and we find that 49 fowls out of 83 have been made insusceptible by inoculation with diluted virus, while only 18 have died; but these inoculations were made, not with the idea of producing immunity with safety, but to learn the effects of the virus diluted to different degrees. By inoculating with a dilution of 1 to 125,000 no effect is produced. A dilution of 1 to 80,000, or probably 1 to 60,000, might be used safely as a first inoculation, and with this the most susceptible birds would contract the lesion

and obtain immunity. The remaining ones should, then, be inoculated with a stronger virus, 1 to 40,000, or 1 to 20,000, perhaps; we are unable to decide exactly without testing it in this manner on a larger number of fowls. If any remain which resist a second time the virus should be gradually increased in strength until all have become insusceptible. How many different strengths of virus this would require in order to make the operation perfectly safe is still to be determined. The varying susceptibilities of fowls to this disease is the great obstacle which we have to encounter; but this must be overcome, as well, with any vaccine prepared by other methods.

The table may lead to some misapprehension in regard to the merits of this method, because from the different susceptibilities of the different lots of fowls operated upon there would appear to be as large a proportion of deaths with virus diluted as 1 to 20,000 as with that diluted as 1 to 10,000, or even as 1 to 1,000. With larger numbers of fowls this discrepancy would disappear. That the effects of the virus gradually lessens with the dilution is very evident on a little consideration. By inoculating with strong virus certainly two-thirds of the fowls would die, and probably a larger proportion, as this is the rate in using virulent blood, while the cultivated virus is much stronger. With a dilution of 1 to 10,000 but one-fourth die, while of 14 inoculated with a dilution of 1 to 50,000, or greater, none have died.

Of ten inoculated with dilutions of 1 to 80,000, or weaker, four, or two-fifths, contracted the local irritation and gained immunity, while the remainder were not affected. Of the 25 inoculated with a dilution of 1 to 10,000, six died and two had mild attacks; that is to say, the virus was too strong for eight. If the proportion should hold good, therefore, and these twenty-five had been first inoculated with a dilution of 1 to 80,000, the ten most susceptible ones would have been granted an immunity, and there would, consequently, have been no deaths when the remainder were inoculated with the dilution of 1 to 10,000. In this case all would have acquired an immunity with two inoculations. This is, perhaps, a more favorable case than we could always expect. I simply call attention to it to give an idea of how the method would work in actual practice.

Duration of the immunity.—It is important to know how long we can expect fowls to remain insusceptible, after they have become so by a mild attack by inoculation or vaccination. If the operation must be frequently performed, it would be too great a tax on animals of so little value. On the other hand, if nearly all should retain their immunity through life, the cost of the operation would be very trifling compared with losses from cholera, which in many sections almost entirely depopulates the poultry yards. Fowls, as a rule, are not kept after they are two or three years old, so that, even if inoculated when young, the duration of the immunity would not need to be very long.

People who recover from contagious diseases, or are vaccinated when

young, frequently retain the immunity thus acquired during life; that is, in many cases, for 50 or 60 years. Cattle and sheep vaccinated for charbon have been shown to be completely insusceptible after eighteen months. With fowls my experiments have been very limited.

The following tests with strong virus were made March 1, 1882:

One hen that had been inoculated May 13, 1881, resisted perfectly.

One hen, frequently inoculated without success during the fall of 1880, resisted perfectly.

One hen, inoculated in the spring of 1881, had a very slight local irritation following this inoculation.

One hen, which had been several times inoculated with strong virus, without success, during the winter and spring of 1881, contracted the disease and died.

One hen, which had an attack of cholera and recovered in August, 1880, resisted perfectly.

Two, inoculated in November, 1881, resisted perfectly.

Judging from the above experiments, the immunity will be retained in most cases a sufficient time to make one successful inoculation all that is required during the lifetime of a fowl.

TESTS OF DISINFECTANTS.—The effect of the different disinfectants upon the virus of the various contagious diseases has never been accurately determined. Until very recently there was scarcely any experimental data to guide us either in selecting a disinfectant or in deciding the strength of the solution in which it should be used. There has, consequently, been much doubt in regard to the efficiency of many of the agents formerly relied upon as disinfectants, and there has been even more doubt in regard to the strength in which these chemicals should be used to give satisfactory results. To give a more substantial basis for practical disinfection a number of experiments have been made which remove all doubt as to the efficiency of a certain number of disinfectants in fowl cholera, and show very clearly the strength in which they should be used.

EFFECT OF PLATT'S CHLORIDES.—A quantity of this disinfectant was furnished to me by the manufacturers, with the request that I should test it in any way that might be convenient. It is represented to be a saturated solution of the chlorides of magnesium, potassium, sodium, zinc, &c.

Experiment No. 11.—Two fowls were inoculated April 24, with virus that had been mixed, for fifteen minutes, with five times its volume of a dilution of Platt's chlorides as 1 to 5.

April 26. The tissues at the point of inoculation are cauterized by the strong chlorides introduced with the virus.

There were no symptoms of cholera as a result of this inoculation.

Experiment No. 12.—Two fowls were inoculated, April 24, with virus that had been mixed, for fifteen minutes, with five times its volume of Platt's chlorides diluted as 1 to 10.

April 26. There is considerable irritation at the point of inoculation from the caustic nature of the disinfectant.

May 7. Yellow urates.

May 9. One dead.

The second was sick some days, but recovered.

Experiment No. 13.—Two fowls were inoculated, April 24, with virus mixed, as in experiment 12, with Platt's chlorides diluted as 1 to 20.

April 26. The irritation produced by the disinfectant at the point of inoculation is less than in the two preceding experiments, but it is still noticeable.

May 7 and 8. Yellow urates and diarrhea; both sick.

May 13. The two birds have recovered, the attack being rather mild.

From these experiments we concluded that Platt's chlorides might be relied upon to destroy the virus of this disease in dilutions of 1 to 5, but that in greater dilutions it was without effect.

As there has recently been considerable discussion as to the value of the chlorides as disinfectants, and as these experiments might be considered too few to allow positive results, I determined to test the effect of this solution by laboratory methods.

Experiments Nos. 14 to 16.—To the sterilized cultivation liquid in three tubes was added, respectively, one-fourth, one-ninth, and one-fourteenth of its volume of Platt's chlorides. A few drops of virus were then added and the apparatus allowed to stand for two hours. At the end of this time, two drops were taken from each tube and placed in the sterilized liquid of fresh tubes. It is plain that if the germs were destroyed by the disinfectant there would be no multiplication in the fresh tubes, while if they retained their vitality this would be very evident from their rapid development. The tubes were kept at 100° F. for twenty-four hours, when the germs that had been treated with the dilutions of 1 to 15 and 1 to 10 were found to have retained their vitality and multiplied at the usual rate, while those which had been in the solution of 1 to 5 had not multiplied and were evidently dead.

The laboratory test and the inoculation experiments coincide exactly, therefore, and the efficacy of the solution may be regarded as definitely determined, as far as regards this virus.

EFFECT OF SULPHURIC ACID.—It had been ascertained by inoculation experiments that a one-half per cent. solution of sulphuric acid very quickly destroys the activity of the virus in this disease. It was desirable to know if a weaker solution might be depended upon to produce the same effect. The following experiments are conclusive on this point:

Experiments Nos. 17 and 18.—To the sterilized liquids of two tubes was added sufficient sulphuric acid to make solutions of one-eighth and one-quarter of one per cent. A few drops of virus were, then, added, and after agitation allowed to stand for an hour and a half. At the end of this time two drops were taken from each tube and placed in a

fresh cultivation apparatus. In twelve hours the germs had multiplied until the liquids in each tube were turbid.

Experiments Nos. 19 and 20.—After the virus, in the foregoing experiments, had been in the disinfectant for twenty-four hours, two drops were again taken from each tube and placed in a fresh cultivation apparatus. After twelve hours' cultivation it was evident that the germs had survived the twenty-four hours' treatment with one-eighth per cent. of the acid. It was only after thirty-six hours that a slight opalescence was seen in the second tube. The stronger solution had, therefore, destroyed most of the germs within the twenty-four hours, or, at least, had greatly enfeebled them. It is plain from this that the one-half per cent. solution of sulphuric acid is the weakest that can be used with safety.

EFFECT OF CARBOLIC ACID.—It was ascertained in previous experiments, by inoculation, that a 1 per cent. solution of carbolic acid very quickly destroyed the activity of this virus. Would the laboratory tests confirm this, and, if so, would a weaker solution prove equally effective? To determine this the following experiments were made:

Experiments Nos. 21 to 23.—The germs were placed in solutions of the strength of 1 per cent., three-fourths per cent, and one-half per cent., according to the same method as in the preceding experiments. After an hour and a half they were placed in cultivation tubes to test their vitality. Those which had been in the 1 per cent. and the three-fourths per cent. solutions were destroyed and unable to multiply, while those which had been in the one-half per cent. solution were still active.

Experiment No. 24.—After the virus had been in the one-half per cent. solution of carbolic acid for twenty-four hours, its vitality was again tested by cultivation. This time it was unable to multiply, and was evidently dead.

A three-fourths per cent. solution of carbolic acid is then sufficient to destroy this virus within an hour or two, while a one-half per cent. solution will accomplish this within twenty-four hours.

EFFECT OF GLYCEROBORATE OF SODA.—A communication having been made to the French Academy of Science, in which this agent was recommended as a more efficient disinfectant than carbolic acid, and at the same time free from poisonous properties, I prepared some according to the formula given in that paper and tested it as below:

Experiments Nos. 25 and 26.—This virus was placed, as in the preceding experiments, for one and one-half and for twenty-four hours in a 1 per cent. solution of the glyceroborate. Its vitality was then tested by cultivation, and in each case it multiplied with its usual activity. The glyceroborate cannot be regarded, therefore, as a very efficient disinfectant.

EFFECT OF CHLOROFORM.—This agent is frequently useful in the laboratory for destroying germs in liquids that are afterwards to be examined for bacterial products, as it can be easily removed by distilla-

tion, thus leaving the original liquid free from any contamination with the disinfectant.

Experiments Nos. 27 to 29.—The living germs were placed for two hours in solutions containing 10 per cent., 3 per cent., and 1 per cent. of chloroform. Their vitality was then tested by cultivation. Those which had been in the 10 per cent. and the 3 per cent. solutions were unable to multiply, while those that had been in the 1 per cent. solution were still vigorous and apparently unaffected.

EFFECT OF CHROMIC ACID.—*Experiment No. 30.*—Virus was placed for two hours in a one-tenth per cent. solution of chromic acid. Its vitality was then tested by cultivation. It multiplied actively and rendered the liquid opalescent after the usual time. Chromic acid in this strength is not a disinfectant for this virus, although other observers have found it efficient with the virus of different diseases.

EFFECT OF IODINE.—*Experiments Nos. 31 to 33.*—The virus was placed in three solutions having respectively the strength of one-fifth, one-eighth, and one-tenth of one per cent. of iodine, and twice this quantity of iodide of potassium to cause solution. After two hours it was placed in cultivation tubes to test its vitality. In each of these cases the germs were destroyed by the disinfectant. In treating virulent blood with this disinfectant, it was found that a one-eighth per cent. solution no longer destroyed its activity when tested by inoculation. Is there a discrepancy here? I am inclined to think that the iodine caused a coagulation of the blood which protected the germs, in this case, from the action of the disinfectant. It is a point worthy of further investigation.

COMPARISON OF SOME RECENT STUDIES OF DISINFECTANTS.

The effect of disinfectants in solutions of a definite strength and acting for a definite length of time, has recently received considerable attention. Koch, of Berlin, has studied the effect of these on the *Bacillus anthracis*; Arloing, Cornevin, and Thomas have studied the effect on the virus of *charbon symptomatique*, known in this country as *black leg* and *black quarter*; Sternberg has experimented with the *micrococcus septicus*, while my own experiments have been made with the micrococcus of fowl cholera. These investigations are giving us data which place the subject of disinfection on a much more secure foundation than it has ever rested upon before, but there is still much to be desired. It will be instructive to compare the results of these different experiments, where they admit of it, but it must be borne in mind that the bacteria of anthrax and black quarter both form spores, and hence these viruses, particularly when dried, resist much stronger agents than do the micrococci.

BICHLORIDE OF MERCURY, OR CORROSIVE SUBLIMATE.—This is considered by Koch the disinfectant *par excellence*. It destroys spores even

in solution of 1 to 20,000. Solutions of 1 to 5,000 to 1 to 1,000 are capable of destroying spores in a few minutes even when applied as spray. The solution of 1 to 5,000 destroys the virus of black quarters either fresh or dried.

IODINE.—Saturated solutions destroy both charbon virus and the virus of black quarter when in a fresh condition. Solutions of one-fifth per cent. destroy the septicæmic virus; those of one-tenth per cent. fail to do this. The fowl cholera virus is destroyed by solutions of one-tenth per cent., which is as far as the tests have been carried.

CARBOLIC ACID.—Two per cent. solutions do not destroy the spores of anthrax; when of a strength of 5 per cent. it destroys them in two days. Its action is very energetic on the rods; when in the proportion of 1 to 850 it entirely prevents their development. Two per cent. solutions destroy the virus of black quarter whether dry or fresh, but require from eight to twenty hours. When in the proportion of one and one-fourth per cent. it destroys the *micrococcus septicus*, but fails when no stronger than one-half per cent. Solutions of three-fourths per cent. destroy the virus of fowl cholera in two hours, and those of one-half per cent. destroy it in twenty-four hours. In oil or alcohol, according to Koch, it loses its antiseptic qualities.

SULPHURIC ACID.—Destroys fresh black quarter virus in 5 per cent. solutions, and the virus of septicæmia and fowl cholera in one-half per cent. solutions.

SALICYLIC ACID.—Said to be without effect on anthrax virus; destroys black quarter virus in solutions of one-tenth per cent. It neutralizes septicæmic virus, when present, in $1\frac{1}{4}$ per cent., and fowl cholera virus at 1 per cent.

BORACIC ACID.—Without effect on anthrax virus; destroys that of black quarter in solutions of 5 per cent., and that of septicæmia in 2 per cent.

NITRIC ACID.—Destroys black quarter virus, when present, to the extent of 5 per cent., and septicæmic virus in one-fourth per cent.

NITRATE OF SILVER.—One-tenth per cent. destroys black quarter virus, either dry or fresh.

PERMANGANATE OF POTASH.—Five per cent. destroys either anthrax or fresh black quarter viruses, and 2 per cent. destroys septicæmic virus.

SULPHATE OF COPPER.—Twenty per cent. destroys virus of black quarter, dry or fresh; and one-fourth per cent. is sufficient with septicæmic virus.

SULPHATE OF IRON.—One-fourth per cent. destroyed the virus of septicæmia, but 20 per cent. failed with black quarter. The sulphates of iron and copper deserve more extensive investigation, especially as poisons for the micrococci.

HYDROCHLORIC ACID.—It requires 50 per cent. to destroy the virus

of black quarter, while 2 per cent. destroys that of anthrax and one-half per cent. that of septicæmia.

CHLORIDE OF ZINC.—Is without effect on the spores of anthrax in 5 per cent. solutions, acting for a month; destroys septicæmic virus in solutions of $2\frac{1}{2}$ per cent.

SULPHUROUS ACID GAS.—Destroys rods, but not dry spores of charbon, when present in the atmosphere, to the extent of 1 per cent.; is without effect on the virus of black quarter; according to Sternberg, 1 per cent. destroys vaccine virus.

BROMINE GAS.—This is one of the few agents, and the only gas, which destroys the virus of black quarter with certainty.

This list includes the most powerful disinfectants which have been discovered up to this time, and all of those which have been tested with more than one virus. It will be noticed that the micrococci of septicæmia and fowl cholera are destroyed with about the same strength of solution, while the two viruses which form spores resist much more concentrated disinfectants. There are many other agents to be tested, some of which have already been tried with one or the other of the contagia above referred to, and it is hoped that the near future will enable us to undertake the disinfection of buildings or grounds with a certainty that we will accomplish this in a reasonable time.

The method of testing disinfectants in the laboratory which I have perfected, and which so far has given results corresponding so exactly with inoculation experiments, will be of very great assistance in this class of studies. Formerly, we were obliged to make a considerable number of inoculation experiments before we could hit upon the exact strength of disinfectant necessary to destroy the virus in a given time. Now, the first trials can be made in cultivation tubes, and two or three inoculations, to confirm these, make our conclusions perfectly safe.

ATTENUATION OF VIRUS AND VACCINATION FOR CONTAGIOUS DISEASES.

When it was discovered that certain fatal and prevalent diseases were produced by an invisible something that multiplied enormously within the affected animal body, the first practical idea was to prevent the formation of this infectious matter and to destroy that which had been already formed. To this end, sick animals were slaughtered when the first symptoms appeared, their bodies were burned or deeply buried, and the buildings and grounds which they had contaminated were disinfected as thoroughly as the knowledge of these agents allowed. The theory was excellent, but the practical difficulties were so great that it was seldom, indeed, that a disease of any considerable virulence could be completely exterminated after an extensive infection had occurred. When large pastures, and particularly those which are unfenced, have become infected, it is a matter of impossibility to destroy the conta-

gious particles which are scattered over them, and in such cases all attempts to combat contagious diseases by disinfection were hopelessly doomed to failure from the beginning.

So much was our helplessness felt under these circumstances that for years attempts have been made to confer upon our animals an additional power of resistance towards the active causes of such diseases. It was known that those which had recovered from one attack of a number of these plagues had acquired an extraordinary power of resisting the inroads of that particular contagion in the future, and the attempt was made to produce mild attacks by inoculating from those animals which were affected with the most benignant forms. The results of such inoculations were so uncertain, however, and in so many cases were fatal, that the practice has never been widely adopted, nor is there any reason why it should be, since it rather increased than diminished the losses.

Inoculation for pleuro-pneumonia has been more successful, because this is a disease of a peculiar character. The germs in this case seem to be unable to distribute themselves throughout the body, but are confined in their reproduction to the organs nearest to their point of entrance. As they are generally received with the inspired air, they attack the lungs, and as these are vital organs their effects upon the individual are extremely fatal. But when these same germs are introduced by a slight wound at the end of the tail, they are so far from any vital organ that, even though they cause intense inflammation, suppuration, or gangrene, their effect on the general health is comparatively insignificant.

Until recently, we were so much in doubt in regard to the nature of the agent causing these diseases that we did not reflect that there was some good and tangible reason why the germs of one disease are able to penetrate to the remotest parts of the body, while those of others cannot advance beyond the organ into which they are first introduced. It is true that we have not yet thoroughly demonstrated the reason or reasons for this difference, but we certainly have made very satisfactory progress towards an explanation; and, by means of this progress, we have been able to institute a new and very effective method for the control of this class of diseases. Thus, some of the most deadly forms of virus have been taken and their characters so completely changed, by certain treatment in the laboratory, that they are no longer able to do more than produce a mere local irritation, which ends by conferring as complete an immunity from that disease as is acquired by the most severe attacks which end in recovery.

These discoveries have wonderfully increased our ability to control such diseases, but the principles upon which they depend are so little understood, and are so extremely important for the work which we have in hand, that it is necessary to enter into a rather extended consideration of the facts which have up to this time been brought to light.

DISCOVERIES OF M. PASTEUR.

When successive cultivations of fowl-cholera virus are made in the broth of chicken muscle, each cultivation being started with germs from the preceding one, and the virulence of the different cultivations is tested, observation demonstrates that there is no sensible variation in the virulence of the liquids produced.

If, now, instead of one cultivation immediately succeeding another, a considerable time is allowed to elapse after one is finished and before the succeeding one is started, a very marked change in this respect occurs. With an interval of eight to fifteen days no variation in the virulence could be detected. When this was increased to a month, six weeks, or even two months, no decided weakening of the virus was observed; still, as the interval increased, it was believed that a certain attenuation could be detected. The fatal result was retarded if the proportionate mortality was unchanged. Certain inoculated fowls languished, very sick, often lame, because the parasite in its propagation across the muscles had affected those of the thigh; cases of pericorditis were of slow duration; abscesses appeared around the eyes; the virus lost the great rapidity of its action. Now, increase the interval between the cultivations to four, five, eight months, or more, before studying the virulence of the newly-produced micro-organisms. This time the scene changes completely. The difference in the virulence of the successive cultivations, which up to this time was not noticeable, or was of a doubtful character, is now shown by considerable effects.

With such intervals between the sowings it is found, on repeating the cultivation, that, in the place of the virulence remaining identical—that is to say, with a mortality of ten fowls from ten inoculations—there occurs descending mortalities of nine, eight, seven, six, five, four, three, two, and one out of ten. Sometimes there is no mortality whatever; that is, the disease is manifested with all the subjects inoculated and all recover. In other terms, by a simple change of the mode of cultivating the parasite, by the single fact of increasing the periods between the sowings, we have a method of obtaining progressively decreasing virulence, and, finally, a true vaccinal virus, which does not kill, produces a benignant disease and preserves from the fatal malady.

It must not be supposed that for all these attenuations things pass with a mathematical fixity and regularity. A certain cultivation, which has waited five or six months for its renewal, may still show a very considerable virulence, while others of the same origin will be already very attenuated after three or four months waiting. We shall soon have the explanation of these anomalies, which are only apparent. Often, even, there is a sudden leap of a virulence still very great to the death of the microscopic parasite, and in an interval of short duration. On passing from one cultivation to the following, one is surprised by the impossibility of all development—the parasite is dead. The death of the para-

site is, besides, an habitual and constant circumstance every time when, before the repetition of the cultivations, a sufficient time is allowed to elapse.

In the course of these phenomena, what becomes of the microscopic organism? Does it change its form or appearance while changing its virulence in so profound a manner? M. Pasteur cannot say that there does not exist certain morphological correspondence between the parasite and the diverse degrees of virulence that it shows, but he admits that up to this time it is impossible to perceive such change; if it really occurs, it disappears to the eye armed with the microscope before the excessive minuteness of the virus. The cultivations are alike for all degrees of virulence. If, at times, one believes that he perceives slight changes they soon seem to be but accidental, for they disappear or are produced in an inverse sense in new cultivations.

What is worthy of remark is that if each variety of virulence is taken as a starting point of new successive cultivations, made at short intervals, the variety of virulence is preserved with its own intensity. If an attenuated virus is selected, for example, which no longer kills but one out ten, it preserves this virulence in its cultivations, if the intervals between the sowings are not exaggerated. What is equally interesting, though in the general sense of the preceding observations, an interval, which is sufficient to cause an attenuated virus to perish, respects a more virulent virus which may, indeed, be attenuated in turn, but which does not necessarily die.

At the point to which we have arrived, an important question presents itself—that of the cause of the diminution of the virulence. The cultivations of the parasite are necessarily made in contact with the air, because our virus is an aerobic being, and deprived of air its development is impossible. It is then natural to ask one's self at the beginning if it may not be in the contact of the oxygen of the air that exists the weakening influence of the virulent property. Might it not be that the little organism which constitutes the virus, remaining in presence of the oxygen of the pure air, in the cultivation medium where it has been multiplying, undergoes certain modifications which would be permanent if the organism was removed from the modifying influence? It might also be asked, it is true, if some principle of the atmospheric air other than oxygen might not interfere in the accomplishment of the phenomenon, the incomparable strangeness of which authorizes any supposition.

It is readily understood that the solution of this problem, in the case of our first hypothesis—that of the influence of the oxygen of the air—is easily accessible by experiment; if the oxygen of the air is the modifying agent of the virulence, we could probably have the proof of it by the effects of suppressing its presence.

To this end let us practice our cultivations in the following manner: A convenient quantity of chicken broth being fertilized with our very

virulent virus, let us fill glass tubes to the two-thirds, three-fourths, etc., of their volume, and then seal these in the lamp. By means of the small quantity of air remaining in the tube the development of the virus commences, a circumstance apparent to the naked eye by an increasing trouble of the liquid; the progress of the cultivation causes, little by little, all the oxygen in the tube to disappear. Then the trouble disappears, the virus is deposited on the walls, and the cultivation liquid becomes transparent. It takes three or four days to produce this effect. The little organism is now sheltered from contact with the oxygen, and it will remain in this state as long as the tube is not opened. What happens now to its virulence? For greater certainty in the study a large number of such tubes are prepared, and simultaneously an equal number of flasks of the same cultivation, but freely exposed to the contact of pure air. It has been stated what occurs to these cultivations exposed to the contact of the air—they undergo a progressive attenuation of their virulence. If the tubes, which have been sealed and sheltered from the air, are opened, one at the end of a month, a second at the end of two months, and so on up to ten months, where the experiments are arrested for the present, the remarkable fact is proved by experiment as their virulence is tested that this is always equal to that at the beginning—to that which served to prepare the sealed tubes. As to the cultivations exposed to the air they are found dead or in possession of the feeblest virulence.

The question which occupies us is then resolved—it is the oxygen of the air which weakens and destroys the virulence.*

The above is a translation, nearly word for word, of the greater part of M. Pasteur's first communication on this subject to the Academy of Science, in October, 1880. In February, 1881, a second communication bearing upon this question was read, of which the portions which interest us at present are as follows:

In recent publications I have made known the first example of attenuation of a virus by the resources of experimentation alone. Formed of a special microbion of an extreme minuteness, this virus may be multiplied by artificial cultivations outside of the animal body. These cultivations, abandoned without possible contamination of their contents, undergo with time modifications more or less profound in their virulence. The oxygen of the air offers itself to us as the principal author of these attenuations; that is to say, of these lessenings of the facility of the microbion's multiplication; for it is apparent that the virulence is confounded in its diverse activities with the parasite's diverse powers of development in the economy.

There is no need of insisting on the interest of these results and of the deductions from them. To seek to lessen virulence by rational means is founding on experimentation the hope of preparing from active viruses of easy growth in the bodies of man or animals vaccinal viruses of limited development, capable of preventing the fatal effects of the former. We have therefore used every effort in our researches in regard to the possible generalization of the action of atmospheric oxygen in the attenuation of the viruses.

* L. Pasteur, de l'atténuation du virus du choléra des poules. *Comptes Rendus*, xci, p. 673.

The virus of charbon, being the best understood, should attract our attention first. At the same time we are going to encounter a difficulty from the start. Between the microbion of fowl cholera and the microbion of charbon there exists an essential difference which does not permit the new researches to exactly imitate the old ones. The microbion of fowl cholera does not appear to transform itself into true germs in the cultivations. In these there are only cells or members always ready to multiply themselves by fission without the particular conditions in which they give true germs being known.

Beer yeast is a striking example of these cellular productions being able to multiply themselves indefinitely without apparition of their originating spores. There exist many mucédines, with tubulous mycelium, which under certain conditions of cultivation give chains of cells more or less spherical, called conidia. These, detached from their branches, are able to reproduce themselves under the form of cells without ever producing, unless there is a change in the conditions of cultivation, the spores of their respective mucédines. These vegetable organisms might be compared to plants that are multiplied by cuttings, and of which the fruits and seeds are not used for the multiplication of the mother plant.

The charbon bacterium behaves itself differently in its artificial cultivation. Its mycelial filaments, if one may speak thus, have scarcely multiplied for twenty-four or forty-eight hours when they are seen to transform themselves, but principally those which are in free contact with the air into ovoid, very refringent corpuscles, which isolate themselves, little by little, and constitute the true germs of the minute organism. Now, observation demonstrates that these germs, so quickly formed in the cultivations, do not undergo with time any alteration from the effects of atmospheric air, either in their vitality or their virulence. I could present to the academy a tube containing spores of a charbon bacteridium formed four years ago, the 21st of March, 1877. Each year the germination of the little corpuscles is tried, and each year this germination occurs with the same facility and the same rapidity as at first. Each year, also, the virulence of the new cultivations is proved, and it has manifested no apparent decline. How, then, try the action of atmospheric air on the virus of charbon with the hope of attenuating it?

The knot of the difficulty is perhaps entirely in this rapid production of the germs of the bacteridium that we have just recalled. Under the form of filament and during its multiplication by fission, is not this organism comparable from all points with the microbion of fowl cholera? That a germ, properly speaking, that a seed undergoes no modification from the influence of the air, is easily conceived, and it is no less easily understood that if there should be a change this would be more apt to occur with a fragment of mycelium. It is thus that a cutting, abandoned on the soil in contact with the air, soon loses all vitality, while a seed under the same conditions is preserved ready to reproduce the plant. If these views have any foundation we are led to believe that in order to test the influence of atmospheric oxygen on the charbon bacteridium, it is indispensable to submit the mycelial development of the little organism to this action, under circumstances in which it could not furnish a single corpuscle germ. From this, the problem which consists in submitting the bacteridium to the action of oxygen consists entirely in preventing the formation of spores. The question thus presented is, as we shall soon see, susceptible of receiving a solution.

It is possible, indeed, to prevent the formation of spores in artificial cultivations of the charbon parasite by different artifices. At the lowest temperature at which this parasite multiplies, that is to say towards $+16^{\circ}$ C., the bacteridium does not form spores, at least for a very long time. The forms of this little microbion at the inferior limit of its development are irregular, ball or pear shaped, in a word, monstrous, but deprived of spores. It is the same in regard to this latter point, at the most elevated temperatures compatible with the cultivation of the parasite, temperatures which vary a little according to the medium. In neutral chicken broth the bacteridium no longer multiplies at 45° . Its cultivation in this is easy, on the contrary, and abun-

dant from 42° to 43° , but equally without the possible formation of spores. As a consequence, a mycelial cultivation of the bacteridium, entirely deprived of germs, may be maintained in contact with pure air between 42° and 43° . Then the following very remarkable results appear: After about a month's waiting the cultivation is dead; that is, when sowed in fresh broth there is complete sterility. The first and second days before that on which this impossibility of development is manifested, and on all the preceding days of the month, the reproduction of the cultivation is easy. So much for the life and the nutrition of the organism. Concerning its virulence, the extraordinary fact is noticed that the bacteridium is already entirely deprived of it after remaining eight days at 42° to 43° , and from that time on; at least, its cultivations are inoffensive for the guinea-pig, the rabbit, and the sheep, three animal species most apt to contract charbon. We are, then, in possession not only of the attenuation of the virulence, but of its apparently complete suppression by a simple artifice in the cultivation. Besides, it is possible to preserve and cultivate the terrible microbion in this inoffensive condition. What happens in this first eight days at 43° which suffices to deprive the bacteridium of all virulence? Let us remember that the microbion of fowl cholera also perishes in its cultivations in contact with air, in a much longer time, it is true, but that in the intervals it undergoes successive attenuations. Are we not authorized to think that it ought to be the same with the microbion of charbon? This anticipation is confirmed by experience. Before the extinction of its virulence the microbion of charbon passes through diverse degrees of attenuation, and, besides, as happens also with the microbion of fowl cholera, each of these conditions of attenuated virulence may be reproduced by cultivation. Finally, since, according to one of our recent communications, charbon does not recur, each of our attenuated charbon microbia constitutes a vaccine for the superior microbion; that is, a proper virus for producing a more benignant disease. What is easier, from this time, than to find in these successive viruses a virus adapted to produce the *fièvre charbonneuse* with sheep, cows, or horses without destroying them, and sufficient to preserve them in the future from the fatal disease? We have practiced this operation with great success on sheep.

The preceding facts suggest a problem of great interest. I speak of the possible return to virulence of the attenuated or even the extinguished viruses. We have just obtained, for example, a charbon bacteridium deprived of all virulence for the guinea-pig, the rabbit, and the sheep. Is it possible to restore to it its activity towards these species of animals? We have also prepared the microbion of fowl cholera deprived of all virulence for fowls. How restore to it the possibility of a development in the Gallinaceans?

The secret of these returns to virulence is, at present, entirely in successive cultivations in the bodies of certain animals.

Our bacteridium, inoffensive for guinea-pigs, is not so at all ages of these animals; but how restricted is the period of virulence! A guinea-pig of several years old, of one year, of six months, of one month, of some weeks, of eight days, of seven, six, or even less, runs no danger of disease and death by inoculation with the weakened bacteridium in question. This, on the contrary, and surprising as appears the result, kills the guinea-pig of one day old. There has not so far been an exception in our experiments on this point. If it is passed, then, from a first guinea-pig one day old to a second, by inoculation of the blood of the first on the second, from this to a third, and so on, the virulence of the bacteridium is progressively increased. Very soon in the succession guinea-pigs of three or four days old may be killed, then those of a week, a month, or several years, and, finally, sheep themselves. The bacteridium has returned to its original virulence. It may be said without hesitation, though we have not yet had occasion to make the proof, that it would kill cows and horses; then it retains this virulence indefinitely, if nothing is done to attenuate it anew.

Concerning the microbion of fowl cholera, when it has come to be without effect on these, its virulence is given back to it by acting on small birds, canaries, and spar-

rows, species which it kills at once. Then, by successive passages through the bodies of these animals, it is made to acquire, little by little, a virulence capable of manifesting itself again on adult fowls.

I terminated my communication of the 26th of October last by remarking that the attenuation of virus by the influence of the air ought to be one of the factors for the extinction of large epidemics. The facts which precede, in their turn, may serve to account for the so-called spontaneous appearance of these plagues. An epidemic that has been extinguished by a weakening of its virus may reappear by the strengthening of this virus under certain influences. The accounts that I have read of the spontaneous appearance of the plague appear to me to offer examples, as the plague of Benhazi, in 1856-1858, when the beginning could not be traced to an original contagion. The plague is a virulent disease belonging to certain countries. In all of these countries its attenuated virus ought to exist ready to take on again its active form when the conditions of climate, of famine, of misery, show themselves there anew. There are other virulent diseases which appear spontaneously in all countries; such is the typhus of camps. Without doubt the germs of the microbia which cause these latter diseases are scattered everywhere. Man carries them on his person or in his intestinal canal without great harm, but equally ready to become dangerous when by the surrounding conditions and successive developments on the surface of sores in debilitated bodies or otherwise their virulence becomes progressively increased.

And here virulence appears to us under a new light, which cannot fail to be alarming for humanity, unless nature in its evolution through past centuries has already met every occasion for the production of virulent or contagious diseases, which is very improbable.

What is a micro-organism inoffensive for man or for any particular animal? It is a being which cannot develop in our body or in the body of this animal; but nothing proves that if this microscopic being penetrates into another of the thousands and thousands of created species it could not invade it and produce sickness. Its virulence, strengthened by successive passages through the representatives of this species, might reach a condition for attacking such or such an animal of large size, mankind, or certain domesticated animals. By this method new virulences and contagia may be created. I am very much inclined to believe that in this way, during past ages, have appeared variola, syphilis, the plague, yellow-fever, &c., and that it is equally by phenomena of this sort that appear, from time to time, certain great epidemics, that of typhus, for example, which I have just mentioned.

Facts observed at the period of variolation (inoculation of small-pox), introduced into science the inverse opinion, that of the possible diminution of virulence by the passage of virus through certain subjects. Jenner held this opinion, about which there is nothing improbable. However, up to the present, we have not met with examples of it, although we have sought them intentionally.*

Still later, M. Pasteur announced that he had been able to keep a cultivation of charbon germs, at a temperature of 42° to 43° , for more than six weeks without entirely destroying its vitality. The experiment commenced the 28th of January, and after the 9th of February the virus no longer proved fatal to adult guinea-pigs. The 28th of February, or after thirty-one days, a new cultivation, made at 35° , from the one that was continually maintained at 42° to 43° , still killed very young mice, but was not fatal to guinea-pigs, rabbits, or sheep. (Mice are more sensitive to charbon than guinea-pigs.) The 12th of March, or forty-three days from the beginning of the experiment, a new cultivation no longer killed either mice or guinea-pigs, except those only a few hours old. We

* *Comptes Rendus*, xcii, p. 429.

were thus put in possession of a bacteridium that it is impossible to bring back to virulence. If this were ever accomplished we might be assured that it would only be by using new animal species now unknown to be inoculable, and which are absolutely different from those which we know at present as apt to contract charbon. In other words, we possess now, and we have a simple means of procuring a bacteridium, developed from the most virulent bacteridium, and which is itself completely inoffensive and exactly comparable to those numerous microscopic organisms which infest our food, our intestinal canal, the dust which we inspire, without occasioning either disease or death.

How unforeseen is this result, when we think that this harmless bacteridium may be cultivated in artificial media with the same facility as the most virulent forms, and that morphologically it cannot be distinguished except by the most unstable characters.*

M. Pasteur has continued his investigations of this interesting question with remarkable perseverance and success. His country is proud of his work, and is giving him every assistance that can aid him in its prosecution, and as a result he has been able to greatly increase the evidence in support of his views. The latest discoveries which he has given to the public are found in his address at the recent Geneva Congress, which is undoubtedly one of his most important papers. The following is a translation of the parts bearing upon the subject in hand :

ADDRESS OF M. PASTEUR AT THE INTERNATIONAL CONGRESS OF GENEVA, SEPTEMBER 5, 1882.

GENTLEMEN: You know that our knowledge of the viruses has recently been enriched by precious gifts, which originated in the researches that I published, in 1880, in regard to the microbion of the disease called fowl cholera.

A virus, even when it is constituted by a microbion, may, without a very marked change in its general morphology, be attenuated in its virulence, preserve this in cultivations, produce germs, and under its new condition communicate a mild disease capable of preserving from the fatal affection which characterizes the action of this virus in its natural condition.†

This precious modification may be produced by a simple exposure of the virus to the oxygen of the air. This action of oxygen is, besides, variable with the temperature at which it is exerted, and with the medium which contains the virus, and in which it originates.

These facts, observed in the first place with the microbion of fowl cholera, have since been extended to the microbion of charbon in a series of studies, in which I have

* L. Pasteur. Le vaccin du charbon. *Comptes Rendus*, xcii, p. 666.

† It is remarkable, however, that the attenuated microbia of charbon and their germs have not the same stability as those of the natural bacterium of charbon found in soils or in diseased animals. There are certain microbia and certain germs of charbon which have been attenuated, and which perish in a few months, while since the 21st of March, 1877, that is, for more than five years, I have tested each year the life and virulence of natural germs formed originally in a mineral solution known as Pasteur's, from sowing with a drop of blood from a sheep which died spontaneously from charbon, and of which the original virulence is apparently always the same. These germs still kill sheep in less than forty-eight hours.

had for collaborators MM. Chamberlain and Roux. About the temperature of 16° and also about that of 43° centigrade (temperatures which are near to those at which the cultivation of the *bacillus* becomes impossible), this bacillus no longer forms spores in various cultivation broths—the broth of fowls, for example. Its exposure to the contact of the air at these temperatures, particularly at that of 42° to 43° , progressively attenuates it, from day to day, until all its virulence is suppressed, and soon it is even destroyed and becomes incapable of further cultivation.*

The certain proof that it is the oxygen of the air to which we must attribute the attenuation of the microbion of fowl cholera has been given by a very simple means. It is sufficient to compare the effects of cultivations preserved free from oxygen with those of similar cultivations exposed to the air. The latter perish in a few months, after having passed through diverse stages of attenuation, while those sheltered from the air, in sealed tubes, show themselves, with this microbion, very virulent after several years. The properties of the *bacillus anthracis*, or microbion of charbon, differ in many respects from those of the microbion of fowl cholera. These differences cause it to be less suitable than its congener for observations of the nature of those which I have just spoken, concerning the action of oxygen. This is due to the circumstance that the microbion of charbon, in the form of filaments, promptly dies in closed tubes free from air. We may overcome the difficulty, and still make evident the influence of the air on the microbion of charbon by the following artifice: Let us suppose, in order to get definite ideas, that a broth is infected and distributed in closed tubes, that it is then placed at 42° to 43° , and that there is death of the tubes in six days, of which we can easily assure ourselves by making cultivations from them every day. Nothing prevents our making from the cultivation of the fifth day, that is, the day before the cultivations in the closed tubes would die, a new cultivation equally protected from the air, and which in turn is kept at 42° to 43° . If the new cultivation dies also in six days, a third may be prepared, which is again distributed in closed tubes, the germs for the fertilization being taken from the five-day old cultivation, and so on. At the same time that these successive cultivations are made protected from the air, parallel cultivations are made in flasks in contact with the air.

Let us compare, now, the virulence of the closed tubes with the virulence of the cultivations of the same days which have been exposed to contact with the air. We notice that the virulence of the cultivations exposed to the air becomes more and more attenuated, and is unable to cause death with guinea-pigs, while that of the cultivations in closed tubes destroys them.†

* We find in a memoir of M. Lœffler, a disciple of Dr. Koch, the following:

The famous experiment of Pouilly-le-Fort, the result of which was surprising, is received with reserve, not without reason. In fact, the basis of the discovery of Pasteur is that the *bacillus anthracis* no longer produces spores at 42° to 43° in neutralized chicken broth; while Koch has demonstrated that it furnishes spores quite as vigorously at 43° on the condition that they are cultivated in shallow liquids (*à plat*) instead of deeply in flasks.

What are they driving at in the laboratory of M. Koch by these insinuations? What matters it that M. Koch, in his experiments, disposed otherwise than ours, believes that he has obtained different results? In what way can it weaken our conclusions?

Have we in truth waited for such a judgment on the remarkably successful experiment of Pouilly-le-Fort?

† The 6th of April, 1881, there was distributed in closed tubes a broth fertilized with the virulent *bacillus anthracis*; a part of the broth was cultivated in contact with the air. The 11th of April the closed tubes no longer started cultivations; the bacillus was dead, reduced to granulations without life. The cultivation of the 10th, that is, the day before death, was sowed in broth that was then distributed in tubes which were sealed in the lamp. A cultivation was also made in contact with the air.

The 16th of April the closed tubes no longer produced cultivations; the culture

The action of the oxygen of the air in the attenuation of the charbon microbion is then quite as incontestible as for the microbion of fowl cholera. The influence of oxygen for the attenuation of the charbon microbion is also manifested by a remarkable peculiarity. It is known that M. Toussaint announced the attenuation of this microbion by the effect of heat alone, and that we could have by this means vaccinal bacteria; but we have recognized that these bacteria do not preserve their original attenuation in cultivations. The first cultivation of heated blood already becomes virulent and fatal. The bacteria attenuated by oxygen, on the contrary, preserve their attenuation in their cultivations.

This difference has a great importance, and it is to it, in part, that we must attribute the difficulty of obtaining practically utilizable charbon vaccines by the method of M. Toussaint. We do not share in the least the contrary opinion recently emitted by M. Chauveau in a note presented to the Academy of Science. On the other hand, there is nothing less sure and regular, whatever precautions one takes, than the effect of heat on charbon blood, even when exerted on a slight thickness and at a fixed temperature.

The principal object of the communication that I have the honor of making to you is to furnish new examples of attenuation by the oxygen of the air, and to demonstrate that we are dealing with a general method of attenuating virus. I commence with a microbion which appeared for the first time under circumstances as interesting as curious.

I have also had for collaborators, in the studies of which I am going to speak to you, MM. Chamberland and Roux, and besides, and more particularly, M. Thuillier. It is in their name and mine that I speak.

The 10th of December, 1880, I was conducted by Dr. Lannelongue, surgeon of the 15th, that is, the day before, was sowed and distributed in closed tubes, and also in a flask which admitted the air.

The 23d of April the closed tubes no longer produced cultivations. The cultures were continued according to the same method.

The 7th of May guinea-pigs were inoculated with cultivations from a tube closed the 21st of April, from one closed the 28th of April, and from one closed the 29th of April. At the same time guinea-pigs were inoculated with cultivations from the flasks that had been in contact with the air, and which were of the same dates, 21st, 28th, 29th of April.

The 12th of May the guinea-pigs inoculated from the closed tubes were found dead, while those inoculated from the open tubes were in good health, and showed no signs of sickness the succeeding days.

With a virulent charbon virus the death of guinea-pigs arrives in forty-eight hours, three days at most. In the example which I have cited it only occurred the fifth day; it is a proof that the virus was a little weakened in closed tubes, and that the temperature must have contributed somewhat to the attenuation. At the same time the great and principal part belongs to the oxygen.

Dr. Büchner has announced that the *bacillus anthracis* may transform itself by successive cultivations into the *hay bacillus à voile thrainé*. I have made a hundred and thirty successive cultivations in the aqueous humor of the eye without ever having seen a trace of this transformation. But the action of the oxygen of the air, as one might suppose, provoked a very slow attenuation of the virulence, quite difficult to recognize. It escaped Dr. Koch as well as the morphologic modifications of the microbion, slight modifications, but nevertheless sufficiently pronounced that in the long run it no longer forms germs. Dr. Koch did not comprehend that, to appreciate very slight diminutions of virulence, one cannot confine himself alone to mice and guinea-pigs, but must use more refractory animals. A large number of individuals of a given species would be killed very nearly in the same conditions and in the same time by successive cultivations, which may, however, have diverse virulences. (*Comptes Rendus*, xci, 1880. PASTEUR, De l'atténuation du virus du choléra des poulés.)

St. Eugénie Hospital, to visit a poor child, five years old, affected with hydrophobia. It had been bitten on the face a month before by a rabid dog. Four hours after its death, which occurred the 11th of December, we inoculated two rabbits with mucus from the palate, diluted with water. The rabbits perished in less than thirty-six hours. In their blood we recognized a special microbion that could be cultivated in a state of purity, and of which the successive cultivations produced the death of rabbits, always with the presence of the same microbion in the blood.

The cadaveric lesions consisted in a partial dilatation of the venous system, in a swelling and lees-of-wine redness of the ganglions of the groin, of the axillæ, of the trachea. The latter are always hemorrhagic. A little saliva covers the lips and escapes at the commissures. The lungs, generally œdematous, are sometimes hepatised. At the point of inoculation, made under the skin of the abdomen, in the cellular tissue this is slightly œdematous and emphysematous.

In an experiment where the instant of the appearance of the virulent organism in the blood was sought, it was seen that nine hours after the inoculation the cultivated blood gave rise to the microbion of the disease, although it was not yet apparent with the microscope; that twelve hours after inoculation it might be recognized with the aid of this instrument. The fever appeared at the same time that the microbion became visible; death occurred thirty-five hours from the inoculation. The temperature only descended below 40° two hours before death. The animal weighed 1^k. 920 at the time of inoculation; 1^k. 730 at the time of death. A diminution of 190 grams in thirty-five hours.

The saliva of the dead rabbits invariably transmits the disease to new rabbits.

Adult guinea-pigs resist perfectly inoculation with this microbion; but it kills in two or three days those which are but a few days old. By pursuing the inoculations from one young guinea-pig to another, the virulence increases, and we easily succeed in killing guinea-pigs one, two, three, and four months old. With the first guinea-pigs the cellular tissue around the point of inoculation offers an œdema bathed with bloody serum, which is often thick and gelatinous; the neighboring muscles are lardaceous, purulent, thickened. It is remarkable that as the number of the animal inoculated in the successive inoculations increases the lesions change in character. The gelatinous degeneration of the cellular tissue, the purulence of the subjacent muscles, disappear to be replaced by a deep redness of these muscles. In these special conditions of exaltation of the virulence, one might believe the guinea-pig dead of acute septicæmia. The microscopic organism is found abundantly in the muscles, quite rarely on the contrary in the blood, and often in so small quantity that they are not always visible with the microscope. There appears to be a change of habitat with the microbion following the augmentation of the virulence.* Here is presented a circumstance very worthy of interest. When the microbion has been increased in virulence by passages through guinea-pigs, it shows itself on the contrary less efficacious if it is used again on rabbits. It is not the only microbion which behaves thus.

We made known the existence of this microbion to the Academy of Medicine of Paris the 18th of January, 1881.

We can see now how great services the study of the microbia may render to etiologic medicine. At the same time that we were making the study of this pathogenic microbion, Dr. Maurice Raynaud, of greatly regretted memory, also undertook from his side, with Dr. Lannelongue, experiments of contagion to rabbits with the saliva of the hydrophobic child of St. Eugénie. Like us, he obtained the death of the sub-

* Dr. Koch and his disciples (*Recueil des travaux de l'office sanitaire allemand*, Berlin, 1881), on the faith of badly-directed experiments, mistake the fact of progressive virulence, indicated in the first place by MM. Coze and Feltz, and later made perfectly plain, in a particular case, by Dr. Davaine. By numbers of our experiments on the conditions of attenuation and return to virulence, it is known to-day, not only that MM. Coze, Feltz, and Davaine have observed justly, but that the particular cases that they have studied are far from being isolated.

jects inoculated; but entirely from clinical observation, leaving aside the possible action of microbia, which might have been introduced into the bodies of the rabbits at the same time as the virus of rabies, he concluded that it was rabies which he communicated to the rabbits. "Until proof of the contrary," he said, "we believe that it is really rabies of which our rabbits have died."

M. Galtier has announced that he has transmitted the rabies of the dog to the rabbit, and has given eighteen days as the average period of incubation. The rabbits of Dr. Maurice Raynaud died much quicker; the average of the period between the instant of the inoculation and the death was only forty-five hours. This difference did not arrest the conclusion of M. Maurice Raynaud. As in his experiments it concerned the transmission of rabies, not from the dog, but from man to the rabbit, he attributed the difference in the periods of incubation to this circumstance. Even before this, the 27th of October, 1879, M. Maurice Raynaud announced having by inoculations of saliva transmitted rabies from man to rabbits. This first conclusion was no more exact than that which I have just recalled. Not that it is so difficult to communicate the rabies of man either to the dog or to the rabbit—we have often done it—but at this period M. Maurice Raynaud had only had under his hands, unknown to him, rabbits dead of the new microbion.

However, if the rapid death of the rabbits in these diverse experiments was due to an entirely new microbion, it might be asked if this new microbion had not some hidden relation with the true microbion of rabies. Was not this salivation of our rabbits and the easy provocation of the disease and the death by their saliva inoculated on new rabbits a strange circumstance?

Besides, was it not very interesting to learn if this same virulence of the saliva of the child dead from hydrophobia at St. Eugénie might be found again with other hydrophobic salivas? The occasion for removing these doubts soon presented itself.

The 23d of February, 1881, M. Percheron, veterinarian, called my attention to a child, six years old, presenting all the symptoms of rabies. She had also been bitten a month before, in the face, by a rabid dog. Her death occurred the same day, the 23d of February, at four o'clock in the evening. The next day, 24th of February, a little salivary mucus was gathered and with it two rabbits were inoculated, one under the abdomen, with the Pravaz syringe, the other on the face, with the lancet. These latter showed no results. The former died after three days. Its blood offered in abundance our new microbion, with its habitual virulence.

At the same time, a smith, aged 49 years, bitten by a rabid dog four months and a half before, died, the 22d of February, at the *Pitié*, under the care of Dr. Brouardel. An hour and a half after his death, several rabbits were inoculated with the saliva of the mouth and the mucus of the palate. Other rabbits had been already inoculated with the saliva taken before death, the day before and some hours before, by Drs. Brouardel and Dujardin Beaumetz. Thanks to the kindness of these learned physicians, I was able to assure myself that not only the rabbits which I had inoculated, but some of those which they had used, were dead from the same microbion that we are considering.

A prolonged and attentive study of the effects of inoculation with human hydrophobic saliva, on rabbits, permits the observation of three varieties of death:

Death from the new microbion.

Death from very abundant purulent disorders, with sloughings of the skin; accidents of the septic order.

Finally, death by the true rabies of the rabbit. This has always quite a long incubation, and is invariably manifested by paralysis of the members which has a duration of twenty-four, forty-eight, or seventy-two hours before death. The aptitude to bite never exists, generally speaking, in the rabies of the rabbit. I have seen, however, an example of it, but a single one in hundreds of cases.

Death by purulent disorders may occur in a few days or in several weeks. In this case it is rare that there is paralysis.

Death by the new microbion is always rapid, at least when there are no purulent complications; in case of these it may be retarded several days.

To sum up, the saliva of hydrophobic persons contains, besides the virus of rabies not yet characterized by a cultivable microbion, a virus formed by a special microbion that may be easily cultivated, and diverse microbia capable of causing death by exaggerated productions of pus, excessive local disorders, and sometimes by the introduction into the blood of common microbia.

In the saliva of children dead from rabies, the new microbion appears sufficiently frequently and abundantly to produce the death of the rabbits with greater rapidity than the hydrophobic virus or the other microbia of the purulent or putrid disorders.

Does the new microbion, discovered in the saliva of persons affected with hydrophobia, exist only in this kind of saliva? This question naturally presents itself to the mind. It was indeed the first to resolve, if we wished to assure ourselves of a hidden relation between this microbion and that of rabies. In case the new microbion should exist in various salivas, it is evident that it would be independent of the virus of rabies.

From the observations to which we have been devoted, it has resulted that the saliva of adult persons, dead from various diseases, does not contain the new microbion, or rather that it has been masked, in our experiments, by the abundance of pus-producing microbia; that, on the contrary, the saliva of children, dead from diverse maladies, has caused the death of rabbits by the microbion in question; that, finally, it has also been found in the salivas of persons in complete health.*

The microbion of the saliva, with which I have just entertained you, is the third virulent microbion which we have tried to attenuate by the action of the oxygen of the air. I desire to present it to you. It is still unpublished, and very interesting by various details of its history. You know already what happens to the cultivations of the fowl-cholera microbion when we pass from one cultivation to the following without allowing a long interval between these cultivations. The virulence of the second cultivation reproduces the virulence of the first without appreciable change, and this is true of successive cultivations. It is only when we allow a greater or less time to elapse between two consecutive cultivations that we observe a diminution in the virulence. In other terms, it seems that the oxygen of the air only has an influence to attenuate a cultivation after this is completed. While the oxygen is employed on the life, on the acts of nutrition of the microbion, its attenuating influence is not exercised in a sensible manner. It is not entirely null, but it escapes ordinary observations.

Our microbion of the saliva comports itself like the microbion of fowl cholera. If we make the cultivations succeed each other every twelve hours, we find in all the cultivations the same virulence; that is, if we take the rabbit as a criterion of virulence, these animals die just as readily and just as promptly from the last cultivations as from the first.

M. Thuillier has had the patience to make, under these conditions, two series of eighty cultivations, and the eightieth killed the rabbits just as quickly as the first.†

*The new microbion has, then, no relation with the virus of rabies. By the details into which I have entered it is sufficiently evident that it was not an easy thing to move without error in all the facts that the above text elucidates. I would dare to say that never, in my former researches, had I pushed further the respect for the principles of the experimental method.

Strange, nevertheless, I have been made to say, notably by the German publication already cited, that the microbion of rabies was no other than our microbion of the saliva. This assertion is entirely gratuitous; it is the contrary that we have established.

†One of the series was made in a vacuum. Is this aerobic microbion equally anaerobic? Was not the cultivation in the vacuum made because of oxygen of the air united with certain oxidizable matters of the broth? This remains to be seen. What is certain is that the cultivation broth became partly decolorized.

To bring out any difference it would have been necessary to sacrifice considerable numbers of rabbits or to operate upon animals more refractory to the virus.

If we compare, now, successive cultivations, after allowing them to remain a greater or less time in contact with the air before passing from one to the other by sowing, the results in certain respects are completely different from fowl cholera. The cultivations perish very quickly. One is greatly surprised to see, when trying to sow a cultivation in a new broth, generally after two or three days of waiting with the mother cultivation, there is complete sterility, and the death of a cultivation occurs the more rapidly as the number of its order increases. A cultivation, fertilized directly by the virulent blood, lives from six to twelve or fifteen days. If with this cultivation we fertilize a second, with this a third, and so on, we notice a prompt diminution of the duration of the life and of the virulence of the cultivations. The eighth will live three or four days, when the twelfth will live thirty hours; the twenty-fifth, twenty-six hours; the forty-eighth and the following, from about twenty to twenty-two hours.

These cultivations, inoculated on rabbits when they are at the end of their life, do not always kill them, and it is easy, then, to notice that among the rabbits inoculated in these conditions, many resist, in the future, the virulent inoculations. The disease, then, does not recur, at least for a long time. However, the rapidity with which the cultivations die renders it very difficult to grasp the precise moment when the sowing of the cultivation will give a suitable vaccine. It would be necessary to greatly increase the duration of the life of the cultivations. We can accomplish this easily by making the medium of cultivation consist of broth and of rabbit blood. The broth suitable for the cultivation of the microbion is that of veal. Broths of chicken, rabbit, beef, and mutton are not suited to it. Two parts of veal broth and one part of pure rabbit blood give, by sowing with virulent blood or of a cultivation in broth even of an elevated order, cultivations which have as much as forty or fifty days' duration. In the last six days the cultivations of broth, infected with this bloody mixture, form a series of cultivations of graduated virulence, all vaccinal in different degrees.

It is still the action of the atmospheric oxygen which modifies the cultivation and progressively attenuates its virulence. The proof is easily given by the means which have already served us; that is, by comparison of the cultivations made and preserved in contact with the air with those in sealed tubes and in a vacuum. While a cultivation made and preserved in contact with the air perishes in a few days in veal broth, the same cultivation made and preserved in a sealed tube or in a vacuum is still virulent after three or four months, perhaps longer. Besides, when death occurs in sealed tubes, the virulence is preserved up to the moment of death.

We are, then, in the possession of three aerobic microbia which may be attenuated by a method that is easily applicable to the preparation of their vaccines: *the microbion of fowl cholera*, *the microbion of charbon*, *the microbion of the saliva*, particularly the saliva in hydrophobia. If I add a fourth in this communication, I think that this new example will suffice to convince you, as I am myself convinced, that a general rational method for attenuation and for the preparation of vaccines has been found, which is in no sense empirical.

It concerns another new virus, encountered for the first time under the following conditions:

The year 1881 was marked, at Paris, by a very serious epizootic of that kind of affection known under the name of *typhoid fever of horses*. The single omnibus company, at Paris, lost more than 1,500 horses. We have commenced some researches in regard to this malady, which, unfortunately for our experiments, has not reappeared in 1882.

By inoculating rabbits with the foamy matter escaping from the nostrils at the moment of death of a horse affected with the above-named disease, the rabbits perish and their blood presents another new microbion in the form of 8 with an elongated

contraction. This microbion communicates to the rabbits a true typhoid fever which kills them in less than twenty-four hours. The lungs are generally hepatized, with pleurisy. Peyer's patches are generally swollen and sometimes resemble raspberries (*framboisès*), and are hemorrhagic. The patch of the ileo-cæcal valve is always very swollen, and oftener hemorrhagic than those of the intestines. The kidneys sometimes hemorrhagic. The liver often a little pale. The animal is soon in a pronounced condition of coma. Four hours after inoculation the fever is already evinced by an elevation of temperature of more than a degree even when death only occurs after thirty-six hours. Peritonitis is quite frequent.*

The attenuation of this microbion takes place when cultivations of it in broth are exposed to the contact of the air, but it is very difficult to seize this, because the period during which it is shown is followed almost immediately by the death of the microbion. In other terms, if a cultivation of this microbion is made and abandoned to itself, in contact with the air, and its virulence tried each day, this proves always fatal for rabbits until, all at once, the cultivation is found dead; that is, it can no longer be cultivated, and is without any effect on the animals. In the cultivations in contact with the air, the microbion passes from virulence to death in fifteen to thirty days if kept at 35°. On the contrary, developed at 35° and left at the temperature of the atmosphere, the cultivations are preserved living for six or eight months or more. In a vacuum the cultivations preserve their virulence at least a year, whether in an incubator or at the ordinary temperature.

In order to seize and fix the attenuation, recourse was had to the following artifice, which recalls that just employed, to demonstrate that it is indeed the oxygen of the air to which is due the attenuation of the charbon microbion at 43°: A cultivation is made from the virulent blood of a dead rabbit and abandoned to itself. Each day a new flask of broth is infected from the first cultivation in order to have as many cultivations as the mother cultivation is days old. There arrives a moment when the seed taken from this mother cultivation proves to be sterile. Having reached this point the cultivation made the day before the death of the first mother cultivation is taken as the mother cultivation of a new series of daily cultures. The second mother cultivation dies in its turn, and, then, a new series of daily cultivations is made by taking for a mother cultivation the fertile cultivation of the day before the death of the second mother culture, and so on.

By this method, we finally obtain cultivations which no longer occasion the death

* The study of this fourth microbion presents a new example of a change of virulence in regard to one species of animals after there is acclimatization, if one may speak thus, in another species.

In July, 1881, when the micro-organism had passed through a small number of rabbits, which it only killed in two or three days, inoculation caused the death of guinea-pigs in five to eight days. The point of inoculation was edematous, with a little pus at the center; the ganglions swollen and hemorrhagic; the lungs hepatized, with pleurisy; the intestines often covered with false membranes; sometimes pericarditis. The spleen rounded at the borders and friable. Peyer's patches having the aspect of a beard not shaved for two days. The microbion in the blood.

In July, 1882, after the passage of the microbion through many rabbits, the inoculation of guinea-pigs only occasioned a local abscess, opening spontaneously, and the pus of which, swarming with the microbion, killed rabbits in less than twenty hours. In short, by numerous passages through the rabbit, the microbion acquired a greater virulence in regard to the rabbit, while losing this in regard to the guinea-pig. In July, 1882, rabbits died even from 1-500th of a drop of virulent blood. They also died, very easily, from eating infectious matter, or from being placed in cages where other rabbits had died from this microbion.

The reader will notice that, in the above text, I do not decide at all the question as to whether this microbion, in spite of its origin, has any part in the production of the disease called typhoid fever of horses.

of rabbits, and limit their action to the production of curable abscesses, the development of which is sometimes enormous. At this time it is easy to observe that we are dealing with vaccinal cultivations, that is to say, that the rabbits which have recovered support, without accidents, the most virulent cultivations of the micro-organism of the typhoid fever of rabbits. The vaccinal cultivations made at short intervals preserve the vaccinal virulence. The proof of the influence of the oxygen of the air in the attenuation is still given by cultivations in a vacuum or protected from the air. They preserve their virulence and only die after a very long time, manifesting their virulence up to the moment of the death of the cultivations.

To recapitulate, it cannot be doubted that we possess a general method of attenuation, of which the application only needs to be modified according to the requirements of the physiological properties of the different microbia. The general principles are found, and one cannot refuse to believe that the future of this order of investigations is rich in the greatest hopes.*

I have given above translations, almost in full, of M. Pasteur's papers on the attenuation of virus, because of the great importance of the subject, and in order that none of his views might be overlooked or perverted, as so often happens when abstracts are made, even with the greatest care.

INVESTIGATIONS OF TOUSSAINT AND CHAUVEAU.

M. Toussaint commenced an investigation of the methods of mitigating virus almost as soon as Pasteur, and though his success was not so great his results are, nevertheless, very interesting; an account of his researches was given with sufficient detail in my last report. Having been prevented by illness from continuing this line of investigations, it was taken up and carried to a very gratifying termination by M. Chauveau.

We know, says this gentleman, that M. Toussaint vaccinates sheep against charbon by inoculating them with virulent blood, heated during a few minutes to a certain temperature. We know, also, by the demonstration that M. Pasteur has given of it, that the elevation of temperature acts in this case by attenuating the activity of the virus—the charbon bacteridium. Does this curious modification, impressed almost instantaneously upon the virulence of this bacteridium, differ essentially in its nature from the attenuated virulence of the bacteridia which develop and multiply at the relatively elevated temperature of 43° to 43°? It passes, at all events, for being much less certain. I propose to demonstrate that this is wrong. Employed according to certain rules that I am going to state, the heating, during a very short time, of blood infected with bacteridia, transforms this fluid into a vaccine, quite as sure as that of M. Pasteur.

The first and principal rule to follow is to practice the heating in a manner to communicate to the blood, almost instantaneously and equally in all its parts, the increased elevation of temperature and to remove it, in the same manner, from this influence. When the quantity of blood

* PASTEUR: De l'atténuation des virus. Communication to the Fourth International Hygienic Congress at Geneva. *Recueil de Médecine Vétérinaire*, 1882, p. 864.

to transform into vaccine is too considerable, all the parts are not uniformly acted upon by a very short heating; the virulent agents of the deeper layers may preserve all their activity and their aptitude to cause a fatal infection, at least when the heating is not too prolonged, in which case we may absolutely kill the greater number of the virulent agents. To avoid this danger, it is necessary to inclose the blood in small cylindrical pipettes, not more than one millimeter in diameter. The extremity of these pipettes is sealed, and the part containing the blood is plunged in a large mass of water raised and maintained at the desired temperature. At the proper time the pipettes are withdrawn and plunged into cold water. Thanks to the small mass of vehicle which contains the virulent agents, these are all heated and cooled at the same time, and with a precision which leaves nothing to be desired.

Another rule ought to be rigorously observed, if one wishes to fully assure the success of the operation; the blood must be gathered under conditions which make it certain that the virulent agents, introduced into the tubes, have all the same vitality, the same activity, and that thus they will be acted upon in the same manner by the heat. This is the case when the blood is taken from a guinea-pig, which has just died after having survived thirty-six to forty-eight hours from the inoculation with a very active virus. Before introducing the blood into the pipettes, this is allowed to form clots, which are broken and crushed in order to extract a defibrinated blood, which is always very rich in virulent rods.

In an hour, with a single guinea-pig, it is easy to prepare the quantity of vaccine necessary to inoculate more than 500 sheep. The inoculation is made with the point of the lancet, charged, by the usual processes of my laboratory, with a very small quantity of virus. Two or three wide hypodermic punctures on the internal surface of an ear are sufficient for an active inoculation.

The vaccine thus prepared ought to be employed immediately, or the day succeeding its preparation at the latest. Experience has taught me that it is, then, quite as harmless and efficacious as Pasteur's vaccine, providing that the heating has been practiced at a proper temperature and during a proper time.

It is the heating at 50°, originated by M. Toussaint, that I have studied with the greatest care. With this temperature it requires about twenty minutes to kill the charbon bacteridium. Heating during eighteen minutes produces an excellent vaccine of a very great degree of attenuation. The attenuation is also marked after heating during a period of ten minutes, but it is not yet sufficient to permit first vaccinations that are absolutely harmless. With greater reason is this the case if the period of heating is reduced to eight minutes. Between these two extreme degrees of attenuation are naturally found a certain number of intermediate degrees, gradually increasing when the period of heating is varied from eighteen to ten minutes.

A first inoculation with weak vaccine (blood heated to 50° during fifteen minutes), and a second inoculation after an interval of ten or fifteen days with strong vaccine (blood heated during nine or ten minutes), preserves sheep from the effects of the most active virus with which they may be subsequently inoculated.*

Messrs. Arloing, Cornevin, and Thomas, whose brilliant investigations of black quarter or symptomatic charbon have added so much to our knowledge of this affection, have succeeded in attenuating the virus of this disease by a method similar to that of Toussaint and Chauveau. The serum from the tumors of this malady is dried in a current of air at the temperature of 32° C. It is then triturated with twice its weight of water, and placed in an incubator for six hours at a temperature of 100° (*i. e.*, the temperature of boiling water) for the weaker vaccine and at 85° for the stronger. The dose of this has a great influence on the result; a certain dose produces a slight disease and vaccinates, while a stronger dose causes a fatal tumor, in which the microbes reacquire all their activity. When operating upon sheep, a centigramme of each virus in a dry state is taken; for cattle, two or three centigrammes, according to the size. This dose is mixed with 100 times its weight of water, and triturated in a mortar until it is in a condition to be injected under the skin with a hypodermic syringe. Six or eight days after the first vaccination the stronger virus is used, and after about fifteen days longer the immunity may be tested by inoculating with five or six drops of fresh serum from a virulent tumor.

The great resistance of this microbion when exposed to a high temperature is something remarkable. It is, however, one of those organisms which form spores, and even in fresh serum many of the rods already contain these, while during the drying process the number of spores are greatly increased. We have, here, undoubtedly the explanation of the survival of this germ after having been exposed for so long a time to the boiling temperature.

Three lots of animals were inoculated with this vaccine, *viz.*, three sheep, two calves, a heifer 18 or 20 months old, and a cow four years old. The inoculation caused with the bovines a slight local swelling which gradually disappeared, and with the sheep a more considerable swelling. The first inoculation caused an elevation of temperature of two to seven-tenths of a degree; the second, from one-half to one degree. All these animals were vaccinated with success, for, inoculated with natural virus at the same time with animals for comparison, they presented slight or insignificant local accidents, while the results of inoculation were grave and almost always fatal with those used for comparison.†

Quite recently M. Peuch has made some investigations on the effect of inoculating sheep with the diluted virus of sheep-pox, which so com-

* *Comptes Rendus*, xciv, p. 1694.

† *Comptes Rendus*, xciv, p. 189.

pletely confirm my experiments with the diluted virus of fowl cholera and my conclusions as to the general application of this method to the different contagious diseases, that I translate M. Bouley's remarks, not having the original paper at hand:

Recalling that M. Chauveau, in his beautiful experiments with vaccine, had transmitted a vaccinal immunity to bovine animals, by subcutaneous injections of vaccine without any specific eruption, either local or general, manifesting itself as a consequence of this method of impregnation; reflecting, on the other hand, on the great richness of the sheep-pox virus in those corpuscular elements which M. Chauveau demonstrated to be the living elements from which sheep-pox originates, M. Peuch asked himself if one could not diminish the accidents of inoculation and reduce the mortality to the smallest possible proportion by properly diluting this virus and injecting it in definite quantities into the cellular tissue.

Such was the idea which inspired him to institute the experiments which he gives an account of in his communication to the Academy of Medicine. The following is a résumé of these:

Of 17 sheep inoculated by subcutaneous injection 8 received a virulent dilution of one-twentieth; 4 received a virulent dilution of one-thirtieth; 5 received a virulent dilution of one-fiftieth.

In no case did it prove fatal, and all acquired immunity from sheep-pox. These inoculations were made the 23d of April, the 17th and 31st of May, and the 24th of June, last, with atmospheric temperatures of 15°, 21°, 25°, and 28°C.

The effects varied according to the extent of the dilution. With the dilution to the twentieth and thirtieth, the inoculation was followed with a generalized eruption which progressed in a regular manner. But with a dilution to the fiftieth, injected in half the dose, the effects produced were less marked. With five sheep, inoculated with 8 centigrammes of this dilution, a single one presented a very mild secondary eruption; three had a pustule at the point of inoculation; with the fifth there was nothing but a slight swelling at the point of insertion. All acquired the immunity.

M. Peuch was led to think, by these results, that one could succeed, with more extended dilutions and smaller doses, in transmitting the immunity in the most harmless manner, the inoculation by this method having no other effect than to communicate the fever of sheep-pox without the cutaneous eruption which constitutes the danger.

The experiments that he is pursuing will, without doubt, soon give a solution, in the sense expected, of this question, which is of such great importance when we consider its relation with inoculation for sheep-pox, but which may have a still greater importance from the standpoint of the general method of prophylaxis by inoculation, even in the two medicines.*

There can be no doubt, after considering the many experiments which I have made with diluted fowl cholera virus, that M. Peuch is perfectly correct in his conclusions; nor can there be any doubt that, by sufficiently diluting the virus, he will be able to convey the immunity, when the only noticeable effect of the inoculation is a swelling too insignificant to be of any consequence. M. Peuch, however, is working under a disadvantage—one so great that in other diseases it has prevented investigators from observing that diluting a virus lessens its effects. This disadvantage arises from the great variation in the number of virulent germs which exist, at any given period of disease, in any animal liquid. For instance, the blood in fowl cholera or charbon may be

*H. BOULEY: *Recueil de Médecine Vétérinaire*, 1882, p. 978.

swarming with micrococci or *bacilli*, as the case may be, or these bacteria may, at the same period of the disease, be so rare that they can scarcely be found with the microscope. The former blood, considered as a virulent liquid, may be a hundred or a thousand times as strong as the latter, and therefore it is very evident that diluting each to the same degree would give contradictory results. And so the lymph of variola differs in regard to the number of germs which it contains, though to a much less extent than blood; but, in any case, the dilutions of such liquids can never be expected to give perfectly definite results. It is only by cultivating the germs at a given temperature, for a given length of time, and in a liquid of a definite composition, that we can be certain of the degree of dilution which it is necessary to give to our virus. In other words, if we have a virus of a standard strength, we may inoculate safely with diluted virus; but, while our virus is of unknown strength, the method by dilution can never be used without accidents.

But do inoculations with diluted virus necessarily produce even the fever of contagious diseases, as Bouley and Peuch evidently believe, in order that an immunity is acquired? I am inclined to a negative conclusion, because fowls inoculated with greatly diluted virus ($\frac{1}{60000}$ or $\frac{1}{70000}$) have an extremely slight local lesion, and apparently no fever whatever. The multiplication of the parasite seems to be local, and the general health is not in the least disturbed; yet, in spite of this, an immunity is acquired equal to that which proceeds from a severe attack of the disease.

The method of conferring immunity by inoculating with diluted virus is no doubt to play an important part in the prophylaxis of the future.

VACCINATION BY INTRAVASCULAR INJECTION OF VIRUS.

Experiments of Arloing, Cornevin, and Thomas.—Although, in a certain number of diseases like charbon and fowl cholera, the introduction of the virus into the bloodstream produces the affection as certainly as when inoculated locally, there are other maladies in which this does not seem to be the case. Symptomatic charbon or black quarter is one of these, as appears from the investigations of Arloing, Cornevin, and Thomas. They say:

If, after having suspended the virus in distilled water and removed all solid particles, this is injected into the jugular vein of the calf, the sheep, or the goat, these animals always survive this inoculation, provided all the necessary precautions have been taken to prevent the microbion from coming in contact with the cellular tissue about the vein or with the walls of this vessel. The inoculated subjects do not present the tumors of the disease; they simply show a greater or less illness, accompanied by loss of appetite and fever (the temperature rising, at the most, $1^{\circ}.9$), and these general symptoms remain only one, two, or three days.

These facts being noticed, it suggested itself to us if the animals which resist the intravenous inoculation have not, *ipso facto*, acquired immunity, as M. H. Bouley and M. Chauveau have observed, with contagious pleuro-pneumonia of cattle. In order to verify this hypothesis we have injected the microbion into the muscles of the sub-

jects which had received an intravenous injection five, eight, ten, fifteen, or twenty days afterwards. None of these inoculations, made up to the present on three calves, five sheep, and one goat, have produced the tumor of this disease. The inoculated product provoked the formation of an abscess in which the microbion preserved its activity.

It is evident, then, that the introduction of the microbion of the tumor of *charbon symptomatique* into the blood of the calf, sheep, and goat confers immunity against the disastrous effects of the intra-muscular inoculation.*

The 26th of September, 1881, the immunity conferred by this method of vaccination was tested on a larger scale. A group of 25 animals of the bovine species was gathered together at Chaumont. Of these, 13 had been vaccinated by intravenous injection the preceding February, while the remaining twelve had never been operated upon, and were used for comparison of the results. All were inoculated in the thigh with five drops of the same virus. The thirteen vaccinated subjects withstood this test without having suffered from its effects, while of the twelve used for comparison nine died, two were very sick, and a single one proved insusceptible. This last one came from a locality where the disease prevailed, and had probably had a light attack at some earlier period of its life.†

Since this experiment a large number of animals have been vaccinated by intravenous injection of strong virus, with very satisfactory results.

Experiments of Thiernesse and Degive.—Very recently Thiernesse and Degive, of Brussels, have made some successful vaccinations for contagious pleuro-pneumonia by intravenous injection of the virus. Of course it has been an established fact for years that the lung exudation, taken at a proper period of the disease, would produce, when inoculated on a susceptible animal, a local inflammation of greater or less intensity, which conferred immunity from the disease in the future. When this virus was inoculated at the dewlap or in other parts of the body containing much cellular tissue and in the neighborhood of vital organs, it produced so intense an inflammation that death was the general result. The dewlap, especially, is spoken of by the French as a *région défendue sous peine de mort*; that is, as a part prohibited under penalty of death. In consequence of this fact, the end of the tail has long been accepted as the proper place for such inoculations, partly because it is a region less subject to violent inflammation, and partly because the inflammation would have much further to progress before reaching a vital organ, and might at any time be arrested by amputating the tail. This method has never been entirely satisfactory, however, for various reasons. In the first place, it frequently produces a suppurating sore, from which the decomposing pus is thrown by the motions of the tail over the persons of the attendants and milkers, and does not always escape the milk-pail itself. Then this pus, containing, as it probably does, the

* *Comptes Rendus*, xci, p. 734.

† H. BOULEY: *Recueil de Médecine Vétérinaire*, 1882, p. 759.

germs of pleuro-pneumonia, becomes a source of danger to all susceptible animals in the neighborhood. Nor is this inoculation entirely without danger to the animal operated upon; a gangrenous inflammation is often the result, which causes the loss of a part of the tail and thus disfigures the animal for life. Sometimes this gangrene progresses toward the body, and, the tail not being amputated in time, it causes the death of the subject. Finally, there were many cases in which no immunity was conferred, and these animals contracted pleuro-pneumonia after exposure in infected stables or to diseased animals.

As long ago as 1868 Bouley attempted to confer immunity by intravenous injection of virus, but not having proper instruments to prevent the virus from coming in contact with the cellular tissue at the point where the vein was punctured, he was unable to escape the disastrous results which follow the entrance of the germs into the tissues of this locality.

Thiernesse and Degive have overcome this difficulty, by adopting substantially the method of Arloing, Cornevin and Thomas with black quarter. The serum is pressed from an affected lung with the hands, then filtered through a fine linen cloth, and drawn up into an hypodermic syringe holding two grammes. The animal is thrown, the external jugular vein exposed, and pressed upon in its lower part to slightly distend it. The needle of the syringe is then adapted, forced through the vascular wall, and the contents of the instrument are slowly injected. A suture with two pins terminates the operation.

The pleuro-pneumonia virus was thus injected into the venous system of four young cattle, in the dose of two grammes, and produced, with three of these, only a slight febrile reaction of short duration; with the fourth an inflammation with exudation was caused, which was followed by a secondary fever of considerable intensity, but of no gravity. Two critical inoculations were afterwards made in the cellular tissue of a region *prohibited by penalty of death*—the dewlap—of these four animals, and produced with all only a slight inflammation. A third critical inoculation, of three of these animals, produced with one a slight inflammatory oedema and with the other two a more pronounced swelling of an inflammatory character, but of no consequence. The insertion of this same virus in the same *prohibited region* of two young animals that had undergone no preliminary inoculation, caused with both a serious inflammation of a progressive character which was promptly fatal.

These gentlemen conclude, therefore, that the intravenous injection of pleuro-pneumonia virus in the dose of two grammes is entirely harmless if the precaution is observed that not a single particle of this liquid comes in contact with the cellular tissue; that the injection of this liquid has the same effect as the caudal inoculation, that is, it invests the organism with a real immunity, which may be demonstrated by inoculation repeated two or three times in a *region prohibited under penalty of death*; and that the immunity can be perfectly acquired without its

being necessary that the infection is characterized by the symptoms and lesions which characterize the natural or spontaneous disease.*

COMPARISON OF THESE METHODS FROM A PRACTICAL STANDPOINT.

There can no longer be any doubt, after considering the facts which are presented in the preceding sections of this report, that with a number of contagious diseases, which are ordinarily fatal in their effects, we can produce as mild an attack as we may desire, either by changing the activity of the virus by peculiar methods of cultivation in the laboratory, by introducing the germs into the body in a peculiar manner, or by limiting the number of these which are inserted. It is not at all improbable that new methods of obtaining this result will be discovered in the near future, and that we shall in this manner be able to protect our animals from all non-recurring contagious fevers. The prospect to the greater part of professional men seems most encouraging; but there is occasionally one who thinks it will be most injudicious to distribute even the attenuated germs of fatal diseases broadcast over the country, for fear that they may be able to regain their fatal activity and again cause the disease with all its virulence. Just how much force there is to this objection we shall probably be unable to decide until vaccination is practiced on a large scale and for a considerable time. There can be no doubt that the better plan will be to exercise great caution in the method which we adopt and the manner in which it is carried into execution. It would, of course, be inexcusable to inoculate the cattle of a whole State for either charbon or black quarter, if neither of these diseases were known to exist, or even if they occurred in a few restricted localities. But if the animals composing a large herd of cattle have commenced to die, one after another, of charbon or of black quarter, it seems to me that there could be no possible objection to the vaccination of the whole herd at once. And so it would appear that in an outbreak of any contagious disease we might properly and safely use a vaccine to protect those animals which were endangered, but had not yet contracted the affection.

If we had a vaccine that would produce an immunity from hog cholera, we could, in case of an outbreak of this destructive plague, vaccinate the hogs of a whole county, or of two or three counties, and in that way arrest its progress by leaving no animals which were susceptible to it within the infected area. Farmers would soon learn to perform this operation for themselves, and, being supplied with reliable vaccine, they would in a short time check the most virulent outbreaks of contagious diseases by rendering the threatened animals insusceptible to the contagion. The vaccinations for charbon and black quarter have been made under these circumstances in France, and, though with the former disease they have been largely practiced, we have so far heard of only good results following them.

* *Recueil de Médecine Vétérinaire*, 1882, p. 971.

As there are a number of different methods of vaccination, it is probably a matter of great importance to decide by experiment on a considerable scale which is the most reliable in its results and which causes the fewest accidents. The method by intravenous injection is undoubtedly restricted to those diseases which have a local manifestation and in which the fatal effects of the disease are not due to the multiplication of the germs in the circulating liquids of the body. It is for this reason that when the germs are injected into the blood-stream, in a limited quantity, they reproduce themselves sufficiently to grant immunity to the animal without greatly affecting the general health or penetrating beyond the vascular walls. If, however, the dose is sufficiently increased, at least in black quarter, the results are different, the germs cross the vascular walls and produce the local and fatal inflammations.*

With such diseases as charbon and fowl cholera, in which the germs multiply throughout the body and cause death rather by their effects upon the blood than upon any particular organ, there seems to be very little if any difference whether the virus is introduced locally into the tissues or whether it is injected directly into the blood. While, therefore, the intravascular injection of considerable quantities of strong virus is not a general method for producing a mild attack, in all contagious affections, the methods of Pasteur and Toussaint with attenuated virus, and my own method with diluted virus, would seem to be applicable to both classes of fevers.

The method by intravenous injection can never be popularized, because of the difficulty and danger of the operation. Not only must the cellular tissue be divided, in order to reach the vein, but the greatest precautions are necessary to prevent the virus coming in contact with the tissues, in which case a fatal result is sure to follow. As we will not for years have a sufficient number of veterinarians to perform such an operation, on a large scale, in this country, the method is one which, while it has great interest for us from a scientific standpoint, cannot be regarded as at all practical in this country. It is, therefore, necessary for us to confine ourselves to the three remaining methods which are still available.

The method of Pasteur has so far attracted most attention, and it has also been more thoroughly investigated and tested upon a larger scale than either of the others. It consists, as we have seen, in allowing a cultivation of the germs to stand, with free access of pure air, for a certain length of time, a period which varies greatly with different germs and very considerably with the same germ.

M. Pasteur assures us that this attenuation is due to the action of the atmospheric oxygen, and he offers a number of experiments which are expected to demonstrate this theory, but it seems to me that he has scarcely succeeded in making this demonstration satisfactory. What first attracts our attention is the fact, which he admits, that no attenu-

* *Comptes Rendus*, xcii, p. 1246.

ation occurs during the multiplication or active life of the microbia. If we make a fresh cultivation of charbon or fowl cholera virus, in full contact with atmospheric oxygen, and in three or four days start a second cultivation by transferring a particle of liquid from our first apparatus to the second, this also being arranged to freely admit the air, and again, after three or four days, we infect a third apparatus from the second, and so on, indefinitely, there will be no attenuation whatever of the virus. With fowl-cholera virus I have made cultivations in this way once a month for a year or more, and at the end of this period there was no appreciable diminution of the virulence, although the germs had been under the influence of atmospheric oxygen for all this time.

The organism is not attenuated, then, while its vitality is kept up by giving it fresh nutriment, and while we allow it to renew itself by reproduction. There is even with the microbion of fowl cholera and charbon no attenuation for a considerable time though all reproduction has ceased; while with the microbion of typhoid fever of horses it is only just as the point of death is reached that the attenuation can be observed. It is plain, therefore, that while the vitality of the germs is retained there is no mitigation of the virulence, even by Pasteur's process.

It suggests itself to us at once, from the sefacts, that the attenuation is really due to that lessened vitality which must necessarily occur, a greater or less time, before the death of any living organism. And this diminished vitality and death must occur, sooner or later, as we know, with all living things, not as the direct influence of oxygen alone, but rather from changes in the nutrition and other internal functions of the living organism itself, which are inseparable from old age; and this period is evidently reached, sooner or later, with the same species according as the conditions of life are favorable or unfavorable.

If the attenuation were due to the effect of oxygen alone, we should suppose that this would consist, essentially, in its becoming accustomed to use so large an amount of this agent that it could no longer obtain sufficient, within the tissues, to allow it to multiply with its accustomed rapidity. And we should expect that when grown in cultivation liquids, with free access of oxygen, it would be as vigorous, as able to resist unfavorable influences, and as long-lived as before any attenuation had occurred. But M. Pasteur has informed us that micrococci attenuated by his method are destroyed at a lower temperature, and that all germs die sooner when the cultivation is abandoned to itself than was the case before the attenuation occurred. There can be no doubt, then, that this attenuation is accompanied with a very considerable loss of vitality, and it becomes a question if this is not the chief factor of the change in virulence, rather than any direct and exclusive action of atmospheric oxygen.

We are told that cultivations in sealed tubes retain their virulence up

to the death of the germ, and that they then pass in an instant from the condition of greatest vitality to death, with no intermediate stages whatever. This conclusion, though somewhat improbable on the face of it, was, of course, necessary to sustain the theory, because if we admit once that the attenuation can occur when no oxygen is present, then such attenuation can no longer be attributed to the action of this agent. But when M. Pasteur attempted to cultivate the *Bacillus anthracis*, in sealed tubes, at a temperature which prevented the formation of spores, he was forced to admit that there was an attenuation even without the oxygen; and this he attributed to the influence of the heat, though, as it occurred much more slowly than when in contact with the air, he still maintains that the great and principal part should be attributed to oxygen. In my experiments with fowl-cholera virus, I have not found that in sealed tubes the germs pass from the condition of full virulence to death at a single bound; but I have found that an attenuation occurs without oxygen, and that this attenuation is more marked in a complete vacuum than in the imperfect vacuums in the partially filled tubes, such as were used by Pasteur.

The truth seems to be that when we place a microbion, which for any reason cannot form spores, in a complete or partial vacuum, or, in other words, when we remove it from the influence of oxygen, its nutritive activity is at once lessened, and it assumes a condition approaching that of the spore. In this inactive or torpid condition it lives longer and the period of decadence is shorter, though none the less real, than when under the continual influence of the air. If we entirely remove the oxygen, or if we keep the cultivation at an abnormally high temperature, the period of diminished vitality and death is hastened, because these are unfavorable conditions of life.

When Pasteur first announced his method of attenuation he felt certain that the vaccines produced were substantial varieties of microbia, that could be depended upon to reproduce themselves with exactly the degree of virulence that had been conferred upon them; in other words, that they would retain their proper vaccinal virulence through an indefinite number of cultivations. This assertion, however, proved to be entirely too absolute; and, now, we are told that "it is remarkable that the attenuated microbia of charbon and their germs have not the same stability as the natural charbon bacteria of soils or of animals affected with this disease. There are certain microbia and certain germs of attenuated charbon which perish in a few months," while virulent germs retained their activity for more than five years. As a consequence of this fact the vaccines prepared in this way are not as reliable as was at first supposed, and it is necessary that these should be not only fresh, but that their degree of virulence should have been recently tested. The greater part of the unsuccessful experiments, thus far reported, seem to have been due to ignorance of this fact.

A considerable number of accidents occurred, in France, when the

vaccinations were begun after the vacations of 1881. One flock of 558 sheep was vaccinated the first time December 6, 1881, and the second time December 18. Within the first six days after the second vaccination nine died of charbon. Another flock of 435 sheep was vaccinated the 4th of April, 1882, the second vaccination being made the 19th of April. Thirteen of these died within six days from the second vaccination. These are not all the accidents which occurred, but are simply examples; while in other flocks the vaccinated animals were found not to have acquired a sufficient degree of immunity. In explaining these deaths M. Pasteur declared that they were due to unlooked for variations in the strength of his vaccines. The weaker vaccine had become so much attenuated that it no longer produced a sufficient degree of immunity to allow the second vaccine to be used with safety. In other cases both vaccines were too weak and no longer produced insusceptibility. The losses, however, were insignificant considering the total number vaccinated. Of 25,000 cattle vaccinated but five had died; and with 300,000 animals, all told, that had been operated upon, the losses were not over 30,000 francs, or two cents for each animal vaccinated.*

Notwithstanding the few unsuccessful vaccinations which we have referred to, the method of M. Pasteur must stand as an accomplished fact, and as one of the most important discoveries of modern times, in relation to the prevention of this class of diseases. The demonstration that certain diseases, produced by microscopic parasites, do not recur; that a mild attack is sufficient to protect against the most active virus in the future, and that such a mild attack can be produced by germs attenuated in the laboratory, must be conceded to M. Pasteur, in spite of the many criticisms that have been aimed against him. His facts are incontestable, however much we may be dissatisfied with his theories.

A certain class of German investigators and their imitators, of whom Koch seems to be the head and front, have undertaken to contest, not only the attenuation of virus, but even the non-recurrence of bacterial diseases, and this rather from theoretical than practical considerations. As Herr Roloff, professor at the Berlin Veterinary School, had asked Pasteur for some of his attenuated virus, the latter seized upon the occasion to ask for an official test by the German department of agriculture, which was granted. The animals provided were 30 ewes, 20 lambs, and 12 cattle. The first vaccination was made by M. Thuillier, Pasteur's assistant, the 5th of April, on half the animals, the remaining half being reserved for comparison. The second vaccination took place the 19th of the same month, and caused the death of three lambs, which had not been sufficiently protected by the first operation. The 6th of May all the animals, both the vaccinated and the unvaccinated, were inoculated with the most active virus, which, in this case, was the blood of a sheep that had recently died from a virulent inoculation. The results of this test, as stated by the commission on May 9, were as fol-

* *Recueil de Médecine Vétérinaire*, 1882, pp. 706-712.

lows: Of the cattle for comparison three were dead and the remaining three sick, and of the twenty-five sheep that had not been vaccinated twenty-four were dead and one sick. On the other hand, the inoculation had not produced the least effect on the twenty-eight vaccinated animals. Evidently M. Pasteur must have been able to produce an attenuated virus in this case, and the vaccination with this attenuated virus must have protected the animals, or in what other manner can such remarkable results be explained? We await with interest the remarks of Herr Koch on this experiment.

So successful were these vaccinations regarded that the German Government decided to repeat them upon a larger scale. A flock of 250 sheep was now put at the disposition of M. Thuillier, who vaccinated them all without any loss, the former experiment having shown that the weaker virus then used was not strong enough in comparison with the second. Thirty of these sheep were then inoculated with strong virus, and all resisted but one. The single death probably would not have occurred but for Thuillier's impatience to return home, he having made the virulent inoculation in ten days instead of waiting fourteen or fifteen, as is necessary for full protection. The demonstration both of the attenuation of the virus and of the non-recurrence of charbon must be looked upon as very satisfactory, however, since of 58 vaccinated animals but one was susceptible to the effects of the strongest virus.* There have been public tests, then, at Pouilly-le-Fort, at Chartres, at Alfort, and at Berlin, and with all the result has been substantially the same; the vaccinated animals have almost perfectly resisted the strong virus, while the unvaccinated have nearly all died from its effects.

After all these public tests, in which about 170 sheep and cattle were used, after 300,000 successful vaccinations in France, Dr. Klein, of England, comes before us with a report which contests both the attenuation of the *B. anthracis* by Pasteur's method and the immunity conferred by vaccine prepared in this way. And this report rests upon experiments with two sheep, six guinea-pigs, and ten mice!

"The results of these experiments," says Dr. Klein, "enable me to say: (a) Animals inoculated with the '*vaccin*' (*premier and deuxième*) are not made immune against fatal anthrax; and (b) both the first and second '*vaccin*' may produce fatal anthrax." To prove these remarkable propositions the results of two series of experiments are given. In the first, two sheep, two guinea-pigs and two mice were inoculated with "*premier vaccin*." In none of these animals was there any change. It would seem that this should at once have aroused suspicion as to the integrity of the virus; for if no effect was produced on such susceptible animals as mice and guinea-pigs, that was a sufficient demonstration that the vaccine was too much deteriorated to produce any degree of immunity in sheep. We have already seen from Pasteur's experiments that this vaccine becomes more and more attenuated with age, and that the

* *Recueil de Médecine Vétérinaire*, 1882, p. 544.

more the vaccine is attenuated in the beginning the more rapidly it deteriorates, and the sooner the germs, even in the condition of spores, lose their power of reproduction. There can be no doubt, then, that in this case the weaker vaccine had nearly or quite lost its vitality.

The two sheep and two guinea-pigs were afterwards inoculated with "*deuxième vaccin*," and this time one of the sheep showed rise of temperature and loss of appetite for two days. The two guinea-pigs were dead of typical anthrax within forty-eight hours. This ought to be at least sufficient evidence that the vaccine did not have the strength of virulent anthrax virus, or the sheep would have been killed as well as the guinea-pigs; but, in spite of this, Klein attempts to show that Pasteur's method does not lessen the virulence of anthrax germs. The four above mice (but two had recently been mentioned in the published reports) were now inoculated with the blood of one of the guinea-pigs, and died of charbon within forty-eight hours. "The two sheep having been inoculated with the '*premier and deuxième vaccin*,' ought have been immune against fatal anthrax. Now see what happened: According to M. Pasteur, the *Bacillus anthracis* of blood of an animal dead of anthrax, when cultivated at 42° to 43° C. for twelve days, loses all virulence and becomes thereby converted into '*vaccin*.' I had grown the *Bacillus anthracis* of blood of a guinea-pig dead of anthrax at a temperature of 42° to 43° C. for twenty-one days, and with this culture I inoculated the above two sheep. The result was that both animals were dead of typical anthrax within forty-eight hours."

In lot B "four guinea-pigs and six mice were inoculated with '*premier vaccin*.' Within forty-eight hours three of the guinea-pigs and three of the mice were dead of typical anthrax. My method of using the fluids for inoculation absolutely precludes any accidental contamination, and hence these must be accepted as perfectly reliable."*

In other words, we are given to understand that because a vaccine, prepared of a strength to suit the susceptibility of sheep, is so strong that it kills half the mice and three-fourths of the guinea-pigs upon which it is inoculated, it must necessarily be unreliable for sheep. Klein used two lots of vaccine; the first was evidently deteriorated, as the "*premier vaccin*" had no effect even upon mice and guinea-pigs; and as this was all that he used upon sheep we cannot be surprised that the results were unsatisfactory. The second lot of vaccine seems to have been of the proper strength, as the "*premier vaccin*" killed part of the mice and guinea-pigs upon which it was tested. But why was not the test made in this case upon sheep, as well as upon the small animals for which it was not intended? Again, the *Bacillus anthracis*, cultivated by Klein's method at 42° to 43° , still killed sheep after twenty-one days, and we are told that these results "must be accepted as perfectly reliable." Indeed! And what of the results with the sixty animals at Pouilly-le-Fort, the thirty-five animals at Chartres, the ninety-two ani-

* *British Medical Journal*, No. 1136, p. 692.

mals at Berlin, all publicly tested, and before men eminent for their scientific standing and their ability to judge just such an experiment? Are we to discard all these positive results with sheep and cattle, and accept the experiments with *two* sheep and a few guinea-pigs and mice which Dr. Klein holds out to us with the assurance that they "must be accepted as perfectly reliable?" Evidently this is asking too much of us.

Whether Klein is able to reach the same result or not, there can be no doubt that Pasteur has attenuated the charbon virus, and that this attenuated virus, properly used, grants a perfect immunity against the most active germs of the disease. There have been too many public experiments to allow us to doubt this fact. But whether this attenuation is practical and easy outside of Pasteur's laboratory; whether his method of vaccination can be generally adopted with safety over a large extent of territory; whether it is advisable to adopt his method at all, are questions of great importance, which up to the present cannot be regarded as settled. There are plainly objections of considerable gravity to Pasteur's method which it would not be wise for us to overlook, and these may be summarized as follows:

1. The time necessary to procure the virus, particularly with fowl cholera.

2. The difficulty of obtaining virus of the proper strength, owing to uncertainty as to the exact time which is necessary.

3. The great liability of attenuated virus to vary in strength within a short time after it is prepared.

4. The necessity of at least two successful vaccinations of the same animal.

5. The quantity of virus required.

The method of attenuation by heat, which has been brought forward by Toussaint and Chauveau, has up to the present been too little tested to allow us to reach any conclusions as to its success in actual practice. According to Pasteur, the very first culture made from virus attenuated in this way regains its full virulence, and destroys instead of protecting animals. Such vaccine must, therefore, be prepared fresh every time it is needed. Again, he maintains that there is nothing less certain and regular than the effect of heat on charbon blood, no matter what precautions are taken, and even if the blood is in small quantity and the temperature fixed. If these assertions prove to be correct, all the objections to Pasteur's method apply equally to this, with the single exception that less time is required to procure the vaccine; and there would be the additional one that we could *never* feel certain that our vaccine was safe and reliable. Judging from Chauveau's great ability as an original investigator, I should expect that this last objection was hardly valid, and that by keeping closely to the process which he has so carefully described a vaccine of very definite strength might be obtained. The acknowledged fact, however, that the vaccine cannot be used for

more than a few hours after its preparation is an insuperable obstacle to the adoption of this method in America.

We have remaining only a single method of general applicability to contagious diseases—the method by dilution, which I have developed and rendered manageable by the experiments detailed in this and in the preceding report. It would seem that there could scarcely be a disease less promising, from its great virulence, for the success of this method than fowl cholera. And, yet, so far as my experiments go, it would appear to be comparatively safe, even in this disease. M. Chauveau has published observations which are sufficient to make it quite apparent that the method by dilution can be applied with equal success to charbon and black quarter; and M. Peuch has lately shown what great services it may render in the destructive malady known as sheep-pox. It ought to be determined at once, if it is not applicable to pleuro-pneumonia, in which case it may be of the greatest service to us, if this plague should reach our western ranges, as it probably will before many years. It might even assist us in exterminating this disease from the districts already infected.

The method by dilution has the following incontestable advantages:

1. The virus may be preserved in its most virulent state, which in all cases is its most stable condition.

2. This virulent virus which is of a definite strength may be renewed at any time and indefinitely without either increasing or diminishing its activity.

3. A given quantity will vaccinate many times the number of animals that could be operated upon with the same amount of any kind of attenuated virus.

4. The vaccine is prepared from the strong virus in a few minutes.

5. The vaccine thus prepared is mathematically definite in regard to its strength.

6. A single successful vaccination produces the complete immunity.

Whether there is more danger of infecting other animals, grounds, or stables by using diluted virus than with the attenuated, or whether it will prove as safe when tested upon a large scale, are questions still to be decided; but up to this time all experiments have been favorable to this method.

OUR ANIMAL PLAGUES AND THE MEANS OF CONTROLLING THEM.

Each year, as we sum up the investigations that we have made, and compare them with what has been accomplished by others who are working in the same field, we are confronted by the ever-recurring question: What practical means are at our disposal for controlling the plagues which annually produce such havoc among our different varieties of live stock? With none of these diseases have we any inter-State regulations for isolating, quarantining, destroying, or otherwise dispos-

ing of affected animals to prevent the dissemination of the contagion, and, with many of them, it is doubtful if such regulations ever will or can be practically enforced.

The annual destruction of our agricultural capital, however, from the ravages of contagious diseases reaches an enormous aggregate; it is increasing with the density of our population and the greater development of the live-stock industry; and it goes on from day to day, from month to month, and from year to year. We may shut our eyes to the loss; we may steadily refuse to acknowledge it; we may console ourselves with the thought that those who call our attention to it are alarmists and are seeking to advance their own interests, and yet, all the time, there is coming up a cry from every section of our country for assistance. The fowls are dying with cholera from the Atlantic to the Pacific, and from the British Possessions to the Gulf of Mexico; hog cholera has spread itself over the great pork producing regions of our country. Cattle are affected on the Atlantic seaboard with pleuro-pneumonia, at the south with Texas fever; in the southwest with charbon and Texas fever, and in all parts of our territory with black quarter, tuberculosis and enzootic abortion. We even find the markets of the world closed, in some cases, against our pork products because of a real or fancied danger from trichiniasis.

Until recently, there has been but little known, even by the veterinary profession, of the nature of these diseases or of any satisfactory methods of controlling them; but the last half dozen years has added so much to our knowledge, and has so materially modified the views of scientists in regard to available means of prevention that it seems desirable, in this report, to make a brief review of those plagues which are doing the most damage and which are most worthy of governmental consideration.

PREVENTION OF FOWL CHOLERA.

If we examine the reports received each year at this Department, from the different parts of the country, we find that chicken cholera is mentioned as being destructive to the fowls in more than half of the counties heard from. The losses are estimated all the way from a few hundred dollars to as high as two hundred thousand dollars in single counties, and, if we consider that the remaining counties are affected to an equal extent, it is not difficult to realize the immense amount of capital that is annually swept out of existence by this plague. It may be that ten millions of dollars would cover the annual loss, but it is about as likely to reach fifteen or twenty millions.

The germs of this disease enter the system by the digestive organs, and they are generally taken with the food. The contagion is spread by means of the excrement of sick fowls or the flesh or other parts of dead ones. Frequently, no doubt, it is carried considerable distances by small birds which are also subject to it.

If the feeding places and runs are kept free from these germs, there is no danger of the fowls ever becoming affected. Of course this could be accomplished by a daily sprinkling with a disinfectant, but this would be entirely too expensive a method to be practical, even in large poultry establishments. The most that we can expect is that when cholera is in a section the poultry owners will watch their fowls, and, in case of sickness, at once remove the affected birds from the flock. The feeding grounds and houses should then be sprinkled with the disinfectant (sulphuric acid 8 ounces, water 8 gallons), and the probability is that no more deaths will occur until the contagion is again introduced from abroad.

There are many cases, however, in which the runs are thoroughly infected and remain so from year to year. Under such circumstances, the poultry houses must be thoroughly cleaned throughout, and the woodwork and floors completely saturated with the disinfectant. Runs must be fenced off for the fowls and these inclosures thoroughly sprinkled. On a small scale this may be done with a watering pot, and on a larger one with a cask or barrel mounted on wheels as with street-sprinklers. The disinfectant costs very little even when several barrels of it are made; it is thoroughly reliable, and, consequently, by proceeding in this way poultry can be raised with the greatest safety, as far as this disease is concerned.

There are people, however, who, from a disinclination to try anything new that requires either expense or exertion, will not watch their fowls or disinfect the houses and feeding places. My experience with farmers leads me to conclude that this class comprises the great majority of poultry raisers, and that, consequently, although we have a very perfect remedy for chicken cholera, it has not been and probably will never be generally adopted. The average farmer wants something different; he is willing to try a remedy, but he is not willing to repeat it very often. If an animal is sick, he thinks one dose of medicine should cure it; and, so, to prevent fowl cholera, he will work for a few hours, or go to a slight expense, but this must be the end of it; he will not trouble himself about the fowls again for a year, if he can possibly avoid it.

This being the fact, and I doubt if any one who attempts to introduce disinfection and close attention will ever contest it, it is perfectly apparent that there is but one way of controlling this destructive scourge. The fowls must be made insusceptible to it; they must be granted an immunity from the effects of the contagion; they must be enabled to run upon infected grounds and to eat food soiled with the active germs of the disease and not suffer from it. Very well; this condition, difficult as it would have appeared no longer than three years ago, is perfectly feasible at this time, thanks to the discoveries made within so short a period. We can change the virus into a vaccine, and at a comparatively small expense we can grant our fowls an immunity

from this disease. There are even a number of methods by which this can be done, each of which is sufficiently perfect at the present time to be practical, but all of which will doubtless be improved when tested upon a larger scale. The choice between them also depends upon their practical working when used upon thousands or tens of thousands of fowls. How this virus or vaccine can be made and distributed so as to reach the consumer in a reliable condition, will be considered in a succeeding paragraph.

What we wish to insist upon is that the investigations of this disease have thrown so much light upon its nature, and the manner of its spread, that we are able to control it in a very satisfactory manner. We have not found a medicine that will cure a diseased fowl, and possibly never shall, but we have discovered something infinitely better and more useful—an approximately perfect preventive.

PREVENTION OF HOG CHOLERA OR SWINE PLAGUE.

This disease is still very prevalent. A large majority of the counties in some States suffered from its ravages in 1880, and we have no reason to believe that the losses from it have diminished in any degree since that year. The investigations of this Department, which have been carried on for a number of years, have shown that its extension was due to contagion; they have enumerated the symptoms and pointed out the diseased organs; they have even indicated the bacterial parasite which is responsible for originating the plague. This is much—it is far more than the most sanguine veterinarian could have reasonably expected to see accomplished in so short a time. Still we lack something. We know our enemy, but we have not conquered him. The mortality from this pest continues unabated; and until we have introduced some effectual means of controlling this, the great practical end of these investigations has not been reached.

In what direction shall we look for this much desired remedy? Can we disinfect the great hog pastures of the West and South? Can we reasonably hope in our present circumstances to quarantine or destroy the thousands of diseased animals now annually to be found in our various States? No, indeed; the thing is plainly impossible. The demonstration of the contagiousness of the disease has enabled our agriculturists to do something to prevent its spread; but without organized efforts the individual farmer is practically at the mercy of the disease.

Our investigations have shown that the plague is a non-recurrent fever, and that the germs might be cultivated; they have even proved that these germs may be made to lose their virulent qualities and produce a mild affection. Surely, we have here sufficient evidence to show that a reliable vaccine might be easily prepared, if we carried our investigations but a little way farther. If we had such a vaccine, if it were furnished in sufficient quantities and of a reliable strength, if it proved

safe in the hands of the farmer, would not our problem be solved? Could we reasonably expect anything more or better for this disease?

M. Pasteur* has recently confirmed our American investigations in a very complete manner. He shows that the disease is produced by a micrococcus, that it is non-recurrent, that the virus may be attenuated and protect from subsequent attacks, and he promises a vaccine by spring.

This should certainly inspire our people with confidence, and it should incite our authorities to give the suffering pork producers the full benefit of these discoveries, at the earliest possible moment. There may be objections to vaccination, and I doubt not these are of a certain importance, but, with hog cholera already distributed over our whole hog-raising territory, these can have but little weight compared with the incalculable benefit that would be conferred by a practicable system of vaccination.

PREVENTION OF TEXAS FEVER.

With Texas fever we have a number of exceedingly difficult problems to solve. The conditions, in this case, are very different from those connected with either fowl cholera or swine plague. With this disease we have an infected and an uninfected part of the country, between which a very definite line may be drawn. The question of greatest importance in this case is to stop the encroachment of the infection upon the previously healthy country. Before this can be accomplished, the border line of the infected district must be traced from the Atlantic coast of Virginia to its southwestern terminus. During the past summer we succeeded in locating nearly two hundred miles of this line in the State of Virginia. At present, it appears as though there would be at least another hundred miles of this line to trace before we can feel certain of the distribution of the infection in this one State. Through North Carolina, South Carolina, and Georgia we already know the location of the greater part of this line with sufficient accuracy. From this westward, however, we are in the greatest doubt, and it will be necessary to examine almost every mile until we are across the Mississippi Valley.

That the disease is advancing toward the North with comparatively rapid strides, in the district so far examined, can no longer be contested. Is this equally true in the Mississippi Valley? And, if so, what is the rate of progress towards the great cattle regions of Kentucky, Illinois, Iowa, Missouri, and Kansas? And how many miles must still be crossed before these regions are invaded? What significant questions are these for one of our greatest agricultural industries; and, yet, who has thought of them, who knows their import, who has any definite knowledge of the situation!

When the infected district is once accurately outlined, the work may

*L. PASTEUR: Sur le rouget, ou mal rouge des pores. *Comptes Rendus*, xcv., p. 1120.

be begun with intelligence and system. There must be laws that will absolutely prevent the driving of cattle from the infected to the uninfected sections, except during the months of December and January. It is probable that cattle may be carried for slaughter over fenced railroads without danger. In the East, this law will not be a matter of inconvenience to any one except the petty cattle dealers of Virginia, who buy stock cattle in the infected parts and represent them to their customers as coming from uninfected counties. It is these unscrupulous men who are responsible for much of the loss which annually occurs, and they are about the only class of people who are not anxious for legislation regulating this traffic and protecting the healthy stock of the State. Fat cattle shipped or driven to market would not be troubled, as the greater part of these go to the boats at Richmond, West Point, or Norfolk, and all these points being within the infected district no harm could possibly be done. In the West, too, I have no doubt but that arrangements could be perfected which would completely protect the country, and at the same time allow all necessary movement of cattle.

Besides the movement of cattle referred to in the preceding paragraph, there is another kind of movement which it is of equal importance to regulate. I refer to cattle running at large in the roads, commons, and woods along the border line of the infected district, and for ten or fifteen miles on each side of it. Such commons are always first infected, and it is by means of them that the infection is so gradually and surely advancing. If what is known in the South as the "fence law" could be rigidly enforced in each of the counties bordering on the infected district, it would probably do more than anything else to check this advance, and, in the opinion of many who live in this section, it would cause the infection to die out in most of the recently-infected counties. Such a law is already in force in many parts of the South, though with no idea of combating this disease, and is regarded as the greatest boon ever conferred on the agriculturists of those sections. The enforcement of such a law, along the line of infection, to arrest the progress of the disease, would then be a benefit rather than an injury, even to those most directly interested. That such a law would be practical and effectual is believed by many of the most intelligent and best-informed gentlemen who have had experience with this plague.

Controlling the movement of cattle in this narrow belt of country would not only stop the encroachment of the infected district, but it would at once put an end to the greater part of those annual losses which are becoming, year by year, more widespread and more disheartening. The losses of cattle in this belt are for the most part due to their pasturing on infected roads and commons, and if the cattle in this district were kept on fenced pastures the losses would stop. Again, the outbreaks north and west of the line are always caused by cattle

which have been brought across it, and hence stopping this movement effectually removes the danger. By these measures, then, the most important indications would be met, and the greater part of the losses would be stopped.

There are, however, three classes of losses, of less national importance, it is true, but still of great magnitude, which would not be affected and which cannot well be controlled by legislation. It is a fact brought out by the investigations of the past year that it is dangerous to move cattle in summer, even from one part of the infected region to another part of it; and this is particularly true in moving them from a recently-infected section to one that has been longer affected. Why this is so, we are not yet in a position to judge, but that the deaths are frequent which follow the moving of cattle for only a few miles, or even from one field to another, we are very certain from a considerable number of cases reported to us from various sections.

A second and very serious loss occurs from shipping fat cattle from just outside of the infected district to the markets of Charleston, Savannah, Mobile, and other large cities of the South, even in winter. Fat cattle are very subject to this disease, and the loss to shippers and butchers is very severe. Again, it is asserted that many cattle affected with Texas fever are killed and their flesh sold as food in these markets, and, consequently, the local boards of health feel inclined to entirely prohibit the entrance of such cattle within their jurisdiction. Such a restriction necessarily acts as a double hardship. It prevents the people of these cities from obtaining the only really good and well-fatted beef that ever comes to them, and it withdraws the best markets that have heretofore been open to the cattle-raisers of a large section of country.

The third class to which I refer is the thoroughbred cattle imported into the South as a means of improving the native animals of this great section. No matter to what part of the infected district they are taken, whether to Eastern or Southern Virginia, to Middle North Carolina, to the greater part of Georgia, Alabama, Mississippi, Louisiana, Arkansas, or Texas, the most of them pay the penalty by dying of Texas fever within the first two years, and the remainder are never safe from its attacks. These deaths have been generally attributed, erroneously, to the effect of change of climate, to the character of the food, to the heat of the sun, and to various other conditions of life; but it results clearly from my observations that, in the immense majority of instances, they are due to Texas fever and nothing else. The acclimatization, so called, is the immunity, more or less complete, which the survivors of these cattle acquire as the result of a mild attack. This class of losses reaches a very large aggregate each year, because the animals are thoroughbred and of great value. I have known of the deaths of a number of animals valued at over one thousand dollars each. And, finally, the indirect loss arising from the discouragement to the development of the cattle industry in this enormous territory is something incalculable.

How such losses are to be prevented in the future is a problem that can only be decided by a complete scientific investigation of this disease. If the cattle which are comprised in the three classes under consideration could be easily and safely vaccinated with an attenuated virus, and thus protected from future attacks, the difficulty would be, in a great measure, removed. It must, therefore, be one of the great objects of these investigations in the future to reach definite conclusions on this important matter.

The feeding of astringent mixtures to susceptible animals which run on infected pastures is believed by many to be a sure preventive of the disease; others do not regard the practice as of any special benefit. If, however, the germs are taken into the system, as usually occurs with charbon, by way of abrasions of the mucous membranes of the mouth and pharynx, caused while eating dry, woody grasses, it is possible that such remedies may have a certain effect. But, while they may change the character of the wounds which they reach in such a manner as to prevent the inoculation of these with germs, it is evident that fresh wounds are continually liable to occur when grazing, and there would be many chances of infection which could not be guarded against in this way. Whether such agents have any protective effect whatever is still an open question, but it is one which it seems to me is worthy of some investigation.

Ruffin's mixture has long enjoyed considerable popularity. It is composed of the following substances:

Salt, $\frac{1}{2}$ gallon.

Sulphur, $\frac{1}{2}$ pint.

Saltpeter, $\frac{1}{2}$ pint.

Copperas, $\frac{1}{2}$ gill.

These are well mixed together and kept, instead of common salt, always within the reach of the cattle.

Other people use lime, sulphur, and salt, and still others red clay, salt, sulphur, and saltpeter.

I will add a few words to these considerations for the benefit of the sufferers in a large belt of territory near the infected district, who have not heretofore understood the nature of this disease or the cause of its ravages. Every outbreak of the plague has been traced, though sometimes with much trouble, to the infection of the roads or pastures by cattle from an infected district. The safety of the stock, in this section, depends upon its being rigidly kept upon well-fenced pastures, to which no strange cattle can ever gain entrance. Thus fenced and secluded, it is safe, but allowed to run for only a single hour upon the dangerous roads and commons, between April and November, and all may be lost. If cattle must be purchased from the infected district, or from uncertain sources, they should only be brought to the farm in December or January. Movements may sometimes be made with safety in November and February, but this is not always the case, and the

prudent farmer will under no circumstances incur the risk. With these precautions hundreds of thousands of dollars might be saved annually by the cattle-owners in the territory referred to.

PREVENTION OF PLEURO-PNEUMONIA.

The management of this disease requires more careful consideration than does that of any other plague which affects our live stock. Although the district already overrun is large, and the losses from its ravages amount to a considerable sum, they are so insignificant as compared with the vast areas and the enormous herds of the South and West, which are endangered by its presence on this side of the Atlantic, that we must of necessity base our action rather upon the welfare of the latter than upon the sufferings of the former. There can be no doubt, then, that this disease should receive immediate attention, and that the only object kept in view should be its complete extinction by the most summary measures at our command. The work is one which should be placed in the hands of a commission of undoubted ability and of sterling integrity, for both qualities will be needed and tested to the utmost before satisfactory results are reached.

We can recommend no temporizing measures in regard to this affection; the only ones applicable are quarantine, restriction of the movement of cattle, slaughter of affected animals, and disinfection. The details for this work have been so often insisted upon, and so thoroughly discussed in recent publications by Professor Law, that it is not necessary for me at present to consider them. We may feel assured, however, that with laws giving the power to inspect, buy, and destroy cattle, and to control the movements of these animals at discretion, there would be no serious difficulty in freeing our country from this destructive plague.

From the standpoint of the investigator, there are two points which it seems important to mention in this place. The virus of this disease needs to be studied more carefully with a view to disinfection. We ought to know how to destroy the virus which has accumulated in a stable in the most thorough manner and in the shortest possible time. As it is, we know nothing definite of the action of disinfectants on the virus of this disease, and our attempts to disinfect buildings or grounds are far from certain or satisfactory on this account.

Again, the virus of pleuro-pneumonia should be more carefully investigated, in order to determine if it is possible to obtain a true vaccine from it—one that might be used to protect cattle from the disease, without danger to susceptible animals. It may not be particularly important just at this time to have such a vaccine, but with so much of the disease in this country it is liable at any time to be carried to the open pastures of the West, and in such a case a safe vaccine would be worth millions of dollars to us. At present it seems that such a vaccine would not be an impossibility, and hence it is worthy of a thorough investigation.

PREVENTION OF CHARBON AND BLACK QUARTER.

These two diseases, formerly supposed to be but different manifestations of the same affection, are now known to be entirely distinct, and are caused by two very different bacterial parasites. They depend more upon the nature of the soil than most other contagious diseases, and consequently the outbreaks are confined, as a rule, to a single farm or to a restricted area of country, though there have been notable exceptions to this. We occasionally hear of a disease in the New England or Middle States which is supposed to be charbon, but generally such accounts come from the West, and more particularly from the southern part of the Mississippi Valley. Undoubtedly there has been much charbon on these rich alluvial lands after the frequent inundations to which they are subjected; but we have no data for estimating either the extent of the infected districts or the value of the animals that have been lost.

Black quarter and black leg is a disease that appears to be much more frequent, and occurs in all parts of the country. The number of animals which die from it is evidently very large, and their value must therefore be considerable.

I have already discussed at considerable length the method of vaccination for charbon and that of inoculation for black quarter, which are now being practiced in France. Both could probably be greatly improved upon and rendered more suitable to our conditions. So scattered are these diseases in our country that a general system of vaccination for them, as is practiced in France, could never be advisable. But wherever there is a farm (and there are undoubtedly many such) where the disease returns from year to year and destroys numbers of animals, it would be proper to use a vaccine and protect the stock from it in the future. Indeed, this seems to be the only means of protection which we are able to extend to the majority of our farmers in the present condition of science.

PREVENTION OF TUBERCULOSIS.

This disease may develop in almost all of our domesticated animals, and, as is well known, it is one of the worst scourges affecting the human race. It is most frequently met with in cattle, and particularly in milch cows. Where large herds are kept in stables, as is the case in the vicinity of all of our large cities, the conditions are most favorable to its propagation, and when once introduced into such stables it frequently attacks a large proportion of the cows that are in them. While inspecting the cow stables of New York City for pleuro-pneumonia, many cases of this kind came under my observation. In one stable, very well ventilated, clean, and comfortable, as far as I could see, where the cows were even allowed to spend the greater part of the day on the pastures, twenty-four out of thirty-one animals were plainly affected with this disease.

It is not entirely confined to stabled cows, however, for I have frequently observed infected herds in country districts, the progress of the disease and the intensity of its effects varying but little from what is seen in cities. Still, my impression is, that it is much less common in the country than among the stabled cows of cities.

The interest in this disease is at present greater than in any other, perhaps, because of the supposed discovery of a bacterial parasite, which may be cultivated and will produce the disease when inoculated. Nor can this interest be too great, for, in the past, this disease has been strangely neglected, notwithstanding the fact that about one hundred and fifty thousand of our people, and probably some millions of dollars worth of animals, annually succumb to it.

Whether the discovery of Koch is confirmed by future investigations or not, it seems to me that there is not a shadow of doubt that this is an inoculable and contagious disease, due to a virus just as specific in its characters as is the virus of chicken cholera, hog cholera, anthrax, black quarter, or pleuro-pneumonia. There have been many doubts thrown upon this view, particularly by English and American investigators, whose researches have, as a rule, been confined to very narrow grooves, and who have completely ignored the experiments of some of the most able scientists of our time. Starting with the unjustifiable and illogical assumption that tubercle is a specific lesion, and can only be produced in one way, they have gone on to show that non-specific inflammations induced by irritation may produce granulations which are anatomically and histologically identical with spontaneous tuberculosis. From this it was not a long stride to the conclusion that tubercles always arise from non-specific inflammations, particularly of the serous membranes.

After a long series of researches, one of our American scientists very recently reaches the conclusion that, "The natural history of tuberculosis, just narrated, is surely against the existence of a special poison, such as now offered again by Koch. It is clearly proved that no infective agent is required to produce tuberculosis." Again we are told that, "In non-scurfulous animals, viz., other than rabbits and guinea-pigs, neither Robinson, nor Wood, and myself, nor any other experimenter, ever succeeded in producing tuberculosis by inoculation, unless done into peritoneum or anterior chamber of the eye. No one, including Koch, ever produced tuberculosis, in animals not predisposed to it, by inoculations into the skin for instance."*

The numerous experiments of both Chauveau and Toussaint are strangely ignored in the above discussion, just as they must always be where such conclusions are reached. How can any one explain the eleven experiments of Chauveau, in which tuberculosis was invariably produced in calves by the ingestion of even small quantities of tuber-

*H. F. FORMAD, B. M., M. D.: The Bacillus Tuberculosis, &c. *Studies from the Pathological Laboratory of the University of Pennsylvania*, No. XI, pp. 9 and 10.

culous matter with the food? How explain the intravascular injections of tubercular material with calves, horses, and asses, which were equally successful? And how, above all, the successful inoculation experiments of the same author made with calves, horses, asses, and mules by inoculations into the subcutaneous connective tissue and even into the skin itself.*

Then there are the numerous experiments of Toussaint—over two hundred, as he assures us—in some of which he has invariably produced tuberculosis in pigs by feeding tuberculous matter, or by inoculations into the subcutaneous connective tissue with tissue-juices, or even the blood of diseased animals.†

Surely such experiments cannot be dismissed as unworthy of consideration. The experiments of Chauveau and Toussaint also throw much light on the tubercles produced by non-specific inflammations. These are really but false tubercles, and inoculations with them never induce tuberculosis. Such a fact should be enough to satisfy the most exacting that there is an essential difference between the two classes of lesions, no matter how similar they may appear to the histologist.

It is not my purpose to discuss this highly important question at greater length in this report. My object is simply to call attention to the disease as one worthy of immediate attention in the investigations of this Department. If tuberculosis is a contagious disease, and generally produced by contagion, then it follows, as a consequence, that it can be prevented by the measures applicable to other similar plagues. The isolation or destruction of diseased animals and disinfection would undoubtedly accomplish much. It is for the investigations of the future, however, to decide what disinfectant can be safely used, and how long and in what strength it must be applied to be effectual.

PREVENTION OF THE ENZOÏTIC ABORTION OF DAIRY COWS.

This disease, which evidently depends upon some form of contagion for its causation, has been estimated to produce an annual loss, in the State of New York alone, of several millions of dollars. At present the affection is widely distributed over the country, and if the estimates in regard to New York were not greatly exaggerated, the annual loss from this source must be equal to or greater than from any other disease. The dairymen have settled down to the conclusion that nothing can be done to check this scourge, and they dislike so much to have its presence in their dairies known that the public hears little in regard to it.

If, as seems likely from our general knowledge of the contagia, the germs of this disease are first scattered upon the stable-floors and upon the grounds where the cattle run, to be taken into the system by soiled

* *Recueil de Médecine Vétérinaire*, 1872, p. 337.

† *Comptes Rendus*, xc, p. 754, and xciii, p. 281.

food, by the dust which rises and floats in the air, or in some similar way; then a thorough and continuous disinfection of the stables and runs should have a very marked effect in controlling it. In one rather large and plainly infected herd I have put this idea into practice with the happiest results—the disinfectant being a 1 per cent. solution of sulphuric acid.

It is very desirable that this trouble should be thoroughly investigated, with a view of revealing its cause and the best disinfectants that are applicable in its treatment. There certainly is no disease of our domesticated animals in which the probabilities are better for valuable practical results.

PREVENTION OF TRICHINOSIS.

This affection, which has served as a pretext for shutting out American pork products from the markets of various European countries, deserves more attention at our hands than it has so far received. Is it true, that with every one thousand hogs examined there will be found ten or fifteen times as many of the American animals infested with this parasite as of those in European countries? If this is the case, why is it that our hogs are infected in so much greater proportion? And by what means can we lessen this infection? Surely these are questions which should be thoroughly investigated.

In the mean time, what can we do to make our pork safe for even those consumers who insist upon eating it uncooked? Are the recent French investigations reliable, from which it has been concluded that, in the vast majority of cases, the trichinæ of well-salted meats are dead, and, therefore, incapable of doing harm? Or, can we easily and cheaply render all of our pork products perfectly safe by freezing, as seems to have been demonstrated by other investigations? If this happens to be the case, we could send any variety of pork products abroad and guarantee it absolutely free from living trichinæ. In years when the over-production is as great as sometimes happens, our ability to do this would be a matter of great importance to us.

WHY THE DEPARTMENT OF AGRICULTURE SHOULD FURNISH VACCINE FOR CONTAGIOUS DISEASES.

From the short review of our animal plagues, and the means of controlling them which is made above, it becomes apparent that we have four contagious diseases, widely disseminated over the country, which may, as it would appear, be prevented by any safe method of vaccination. There are a number of methods of preparing vaccine and of performing the operation, but which of them should be recommended by this Department can only be decided after a very extensive series of experiments. And such experiments can only be made by the Government because of the expense connected with them.

Suppose, however, we have decided that, for example, the method of Pasteur is, all things considered, the best for us to adopt. Suppose we say to our farmers all over the country that they can prevent chicken cholera, and hog cholera, and anthrax, and black leg by using a vaccine that is made according to the directions of this eminent scientist. Can we possibly expect any practical results to follow such an announcement? No, indeed; because no one can vaccinate against these diseases unless they are supplied with the vaccine. And this is so difficult to make that, outside of Pasteur's laboratory, no experiments in the production and use of it have been published as successful, up to this time, with the single exception of my own. We can hardly expect this vaccine to be supplied in a reliable form by any individuals, therefore; and, as in all cases, it seems to deteriorate within a few weeks after it is made, we cannot draw our supplies from M. Pasteur's laboratory with any confidence of satisfactory results.

There is, consequently, but one course open to us if we desire any practical results from these recent and valuable discoveries: the Department of Agriculture must establish a laboratory to which the people can apply at any time and be certain of receiving fresh and reliable vaccine.

It may be that in the near future it will be possible to protect our animals from Texas fever by vaccination, and, if so, this would be an additional work for such a laboratory to carry on. If this system should be generally adopted, however, the diseases which I have mentioned, and to which prevention by vaccination seems to be more particularly applicable, would give ample employment to a larger force than is likely to be devoted to it for some years.

At present we need such a laboratory for making vaccine for our own experiments with the different methods, and for working out the many practical details which are necessarily connected with any method that should be adopted and carried forward on so large a scale as this must be to meet the demands of our extensive territory. This can only be accomplished in a laboratory fitted up for this special work, and hence these discoveries can never be made practical until the Department provides such facilities.

To sum the whole matter up in a few words, the investigations of the last few years have made it perfectly apparent that our animal plagues are due to bacterial parasites; that these, with some of the most important diseases, are widely disseminated, and can only be combated by making our animals insusceptible to them; that all animals, even the most vigorous and the best cared for, are susceptible until they have had a mild attack, which alone seems sufficient to grant them immunity. The tendency of all recent research has been to produce such a mild attack by means of attenuated virus, and to thus put the animals in a position to encounter the most virulent germs without danger. To adopt this method of prevention we must have the prepared virus, and to get

reliable virus at a reasonable expense there seems no other way than for the Department of Agriculture to furnish it, as it now does many varieties of seeds.

NECESSITY OF FURTHER INVESTIGATIONS.

I have endeavored to make it a prominent feature of this report to show that these investigations should not stop short of practical results; such results are now almost within our grasp, and to obtain the benefits of them we have only to give a slightly different turn to our methods of work. So far the investigations have been in many respects superficial; we have been feeling our way that we might learn what ought to be done and what could be accomplished. We should now fill up the gaps that are left by a thorough investigation of all the details and all the principles involved. Our recommendations can then be safely made, because they will be derived from a basis of experimental evidence that is perfectly reliable, and the indications of which cannot be mistaken.

Investigation of the nature and characters of virus.—In fowl cholera, anthrax, and black quarter the virus has already been quite thoroughly studied. With hog cholera we can scarcely have any doubt as to the form of parasite, but we know almost nothing of its natural history or the conditions necessary for its existence. With Texas fever we are still without any definite and satisfactory information, even in regard to the essential cause, though it is probable that this was discovered in the investigations of the past summer. In regard to the virus of pleuropneumonia, tuberculosis, and enzootic abortion, we have almost no knowledge whatever. The first step in the study of this class of diseases being the isolation and cultivation of the contagious germs, it is absolutely essential for us to continue this line of research.

The study of disinfectants.—This becomes possible when we have found the virus and are able to produce a disease by inoculation. We can determine the exact effect of any disinfectant in any strength and acting for any length of time by placing the germs in the solutions, with which tubes are filled, and afterwards transferring them to the fresh liquid in a cultivation apparatus. After definite results are obtained in this way they can be confirmed and rendered free from doubt by inoculation experiments. At this time we are far from knowing what disinfectants are most effectual in the different diseases which we have to contend with. A single point recently brought out by both German and French investigators shows how important this matter is. Carbolic acid has long been considered our most available disinfectant in many diseases. It has sometimes been used dissolved in water, sometimes in oil, and sometimes in alcohol. Now, the investigations to which I refer show that the aqueous solution is alone disinfectant, and that when dissolved in oil or alcohol it becomes harmless to disease germs. This is only one

of very many similar details which must be elucidated before we are in a position to grapple successfully with our animal plagues.

Experiments in regard to the treatment of sick animals.—The treatment of sick animals or of people suffering from contagious diseases has never been satisfactory. Why this is so we are hardly in a position to judge, but it seems to be partly, at least, because our ideas as to what is to be accomplished are radically wrong. There are, however, so many cases in which medical treatment, even with animals, is very desirable, and the effect of any real discoveries in regard to treatment would be productive of so much good to the human species, that it seems to me we can hardly lose sight of this line of investigation when the opportunity occurs for its study. A combination of stimulants and disinfectants seems to promise the best results, but these can be selected only after a thorough study of their peculiar properties. Such studies, it is needless to say, are yet for the most part to be accomplished.

Experiments to decide as to the best method of attenuating virus.—Having already discussed this matter at considerable length, I only mention it here to complete this section of my report. Researches of this kind must be made before our farmers can obtain any practical benefit from the recent discovery of the possibility of generalizing vaccination.

Researches to discover methods of vaccination applicable to hog cholera and Texas fever.—These two diseases are, perhaps, the most important that we have to contend with. Vaccination is plainly indicated with both of them if it can be made practical. It must, consequently, be one of the primary objects of future investigations to develop this line of inquiry in a most thorough and comprehensive manner.

Investigations to learn the present distribution of Texas fever.—This work has not yet been completed in Virginia. There is probably a hundred miles of the border line of infection still to locate. It is essential that this sanitary survey should be completed before the State of Virginia can enact effectual laws to check the extension of this plague. Such laws are extremely desirable, as the most important advance of the disease in the Eastern States is occurring here. The people of Virginia are extremely anxious for such laws for their own protection, and when they have sufficient information to allow these to be framed intelligently, I have no doubt but that prompt action will be taken in regard to this important matter.

As to the distribution of this disease in the States of Tennessee and Arkansas, we are almost in complete ignorance. I have some facts which indicate that it is gaining a foothold in Missouri, but I refrain from more than mentioning them, because they are too indefinite. It is sufficiently evident, I think, that this line should be at once traced across the Mississippi Valley, in order that the Western States may adopt timely measures to protect their enormous cattle interests.

Studies of the nature of immunity and how produced.—The one point connected with contagious diseases which is mysterious beyond all

others, and the elucidation of which promises the most important practical results is the nature of the immunity conferred by vaccination, and the essential manner in which this insusceptibility is produced. There are those who believe they have produced immunity by hypodermic injection of liquids deprived of living germs, and all our present knowledge seems to point to this as possible. My experiments in this line have always failed, however, and yet they have been more numerous and probably more carefully conducted than those of any other investigator who has studied this question. It is a subject the importance of which cannot be overestimated, and I trust that the researches of the near future will remove the mystery which conceals it. If insusceptibility could be as easily produced by chemical products as by living germs, all the benefits of vaccination would be obtained with none of its objections. Surely there is no more important question that could engage our attention.

The distribution, preservation, and destruction of virus on fields and commons.—With Texas fever, particularly, we need much information in regard to how the germs leave the body, and why they are, as a rule, distributed by apparently healthy cattle and not by sick ones. We ought to know how the germs of this and other diseases are preserved on fields and commons, often under conditions which at first sight would appear destructive to them. Thus, Texas fever virus seems to be destroyed by frost, and yet in some way it survives severe winters. This and hog and chicken cholera are caused by micrococci, organisms of a limited period of existence, which die in the course of a few months unless they are in some medium suitable for their reproduction. How are these viruses preserved for weeks and months in yards and fields and woods?

Again, the destruction of the infection is a matter of great importance. With none of our most common diseases does the bacterial parasites form spores, and hence they are peculiarly susceptible to all unfavorable conditions of life, and to the action of disinfectants. Can we not destroy the infection of these diseases, then, by burning over the pastures, by plowing them late in fall and exposing the soil to atmospheric influences during the winter, or even by keeping all animals from them for one or two summers? Such questions are of especial importance to those who attempt to keep thoroughbred cattle at the South.




Respectfully submitted.

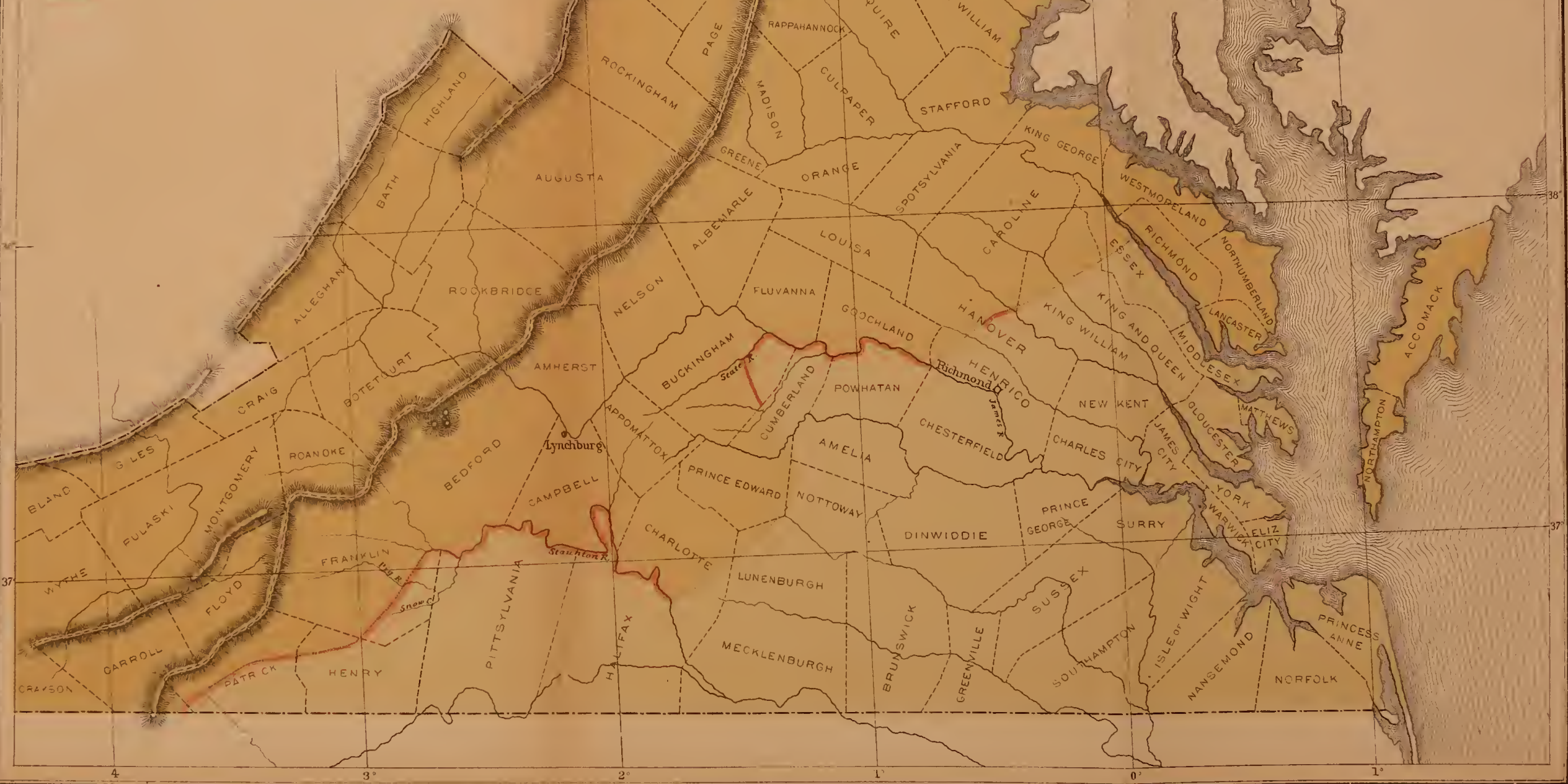
D. E. SALMON, D. V. M.

ASHEVILLE, N. C., February 1, 1883.

TEXAS FEVER IN VIRGINIA.

By D.E. Salmon, D.V.M.

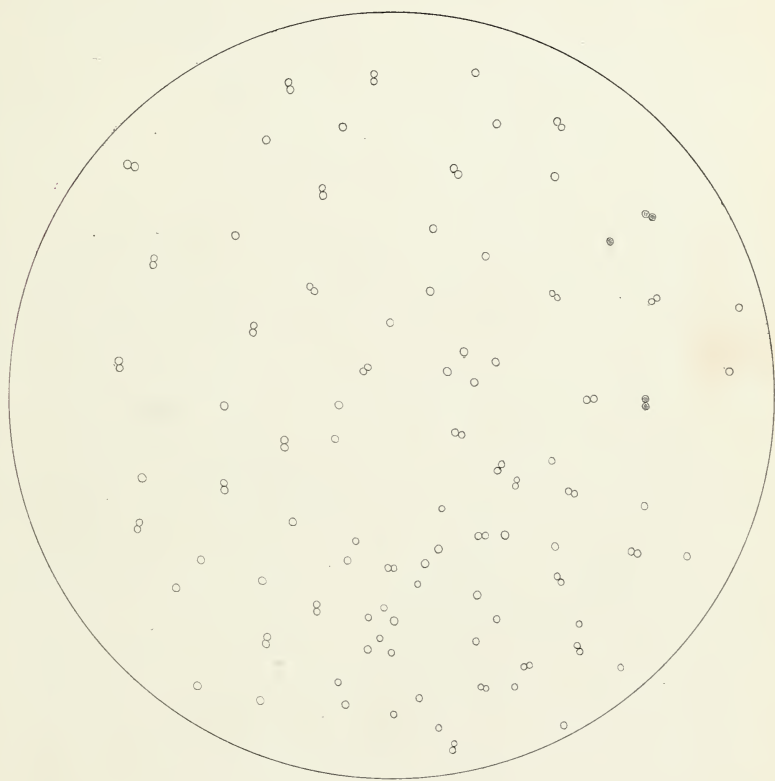
-  Free from permanent infection.
-  Permanently infected.
-  Part of the border line definitely established.



TEXAS, OR SOUTHERN CATTLE FEVER.

Investigations of D.E. Salmon, D.V.M.

Plate I.



Texas Fever: Micrococci from the spleen as seen in the cultivation liquids. x1000.

INVESTIGATION OF SOUTHERN CATTLE FEVER.

REPORT OF DR. H. J. DETMERS.

Hon. GEO. B. LORING,

Commissioner of Agriculture :

SIR: In your favor of December 27, 1881, you requested me to at once proceed to Texas and commence an investigation of "splenic" or "Texas" fever of cattle. You said you desired a thorough examination as to the history and characteristics of this disease, and left it to my own judgment as to the best locality for the prosecution of the investigation. Immediately on receipt of your letter, which arrived on December 29, I made my arrangements to comply with your request, and was thus enabled to leave Champaign on January 2, 1882. As far as circumstances permitted, I endeavored to execute the plan outlined in my letter to you, dated November 10, 1881.

When accepting the commission to investigate so-called Texas cattle fever, I was well aware of the great difficulties to be overcome. I knew I had to deal with a disease enveloped in mystery, which, it was said, does not affect Texas cattle, and of which a Southern origin is denied by a great many cattle men in Texas. Being a stranger in Texas, unacquainted with the country, and having but limited knowledge of the mode and manner in which the large herds of Texas cattle are raised and kept, or of the conditions which surround them, and having neither connections nor reliable acquaintances in the South, of whom I might learn where and when importations of Northern cattle are made, I had first to take some preliminary steps to procure information, and to become acquainted with the people of whom I had to ask favors, before I could hope to meet with success. Then, after I had become sufficiently acquainted with all the factors I had to deal with, I found it necessary to gain as thorough a knowledge as possible of the disease in question, its morbid process and its workings, and its means and manner of propagation, &c., before I could expect to meet with any success in devising remedies or means of prevention. What I have been able to accomplish, and what yet remains to be done, will become apparent in the following pages. As the investigation is not yet completed, I have made no attempt to give a complete treatise on, or a methodical description of, so-called Texas, Spanish, or Southern cattle fever in its various phases, but have restricted myself to giving a simple account of what has so far been ascertained. To give a complete description, methodically arranged, will be in time when the investigation is completed, or

my plan as laid down in my letter of November 10, 1881, is fully executed.

Before I commenced my investigation, I corresponded with, and called upon, the president of the Illinois State Board of Agriculture, the editors of some of the large daily and agricultural papers in Chicago and Saint Louis, and some of the members and officers of the Missouri and Kansas State Board of Agriculture, and asked those gentlemen to furnish me with such information in regard to importations of Northern cattle into Texas, and outbreaks of so-called Texas cattle fever in the North, as might come to their knowledge. I well knew that in Texas, except, perhaps, in extraordinary cases, only the imported Northern cattle become affected with the Southern or Spanish fever. Knowing also that Southern Kansas, particularly in the vicinity of the shipping points of Texas cattle, is, every fall, to a greater or less extent, invaded by the Southern or Texas cattle fever, and suffers considerable loss on account of that disease, I thought it might be best not to go directly to Texas, but to make first a trip through Southern Kansas, along the line of the Atchison, Topeka and Santa Fé Railroad, and to search for information in those towns on that road to which Texas and Indian Territory cattle are driven on the hoof, and from which they are shipped by rail. I also called on several gentlemen who handle Texas cattle, and are interested in the cattle trade at other places.

My success, however, I must admit, was not quite what I expected. In the first place, although the winter was a very mild one, I arrived too late in the season to find much cattle fever in Kansas; secondly, as the winter of 1880-'81 had been a hard one, but very little cattle fever had occurred in Southern Kansas in the fall of 1881; and thirdly, being an entire stranger in that section of the country, I suppose I did not always strike the right men and the right places. Still, I collected some valuable information, and my trip through Kansas was not altogether in vain. But as its results were not up to my expectations, I very soon went directly to Texas, and arrived at Austin, the capital, on January 12. Among other things, my brief trip through Kansas, and my conversations with cattle men and cattle dealers taught me that it was necessary to gain the confidence and good-will of the men who own the cattle ranches and handle Texas cattle, if I expected to obtain any reliable information, and to make my investigation a success. Nearly all the ranch men and the cattle dealers and drovers, but particularly all those interested in the Texas cattle trade, are inclined to look with suspicion upon any stranger who mentions Texas cattle fever; they keep very distant and do not commit themselves, or do not say anything until their confidence has been gained, and they are fully convinced that the person making inquiries is their friend, and has no intention whatever to harm them, or to collect statistics which possibly might damage their trade. When in Austin, I at once called upon His Excellency Governor O. M. Roberts, for the purpose of informing him of my business and of asking his kind

assistance and indorsement. As he was sick in bed I could not see him at my first call, but had to wait till the 14th, when I saw him and explained to him the object of my mission. He gave me some valuable information. He had known the disease, misnamed "Texas fever," as he expressed himself, under the name of "murrain" or "bloody murrain," for over forty years, or since 1841, when he observed it among Northern cattle in Saint Augustine, Texas. Out of thirty head of imported Northern cattle only one animal survived. Governor Roberts expressed the opinion that Southern or Spanish cattle fever—to the name Texas cattle fever the governor very much objects, and gives good reasons—is to a certain extent caused by a change of climate and locality, and by travel, that is, such travel as causes a considerable increase in the products of waste in the animal system. The disease, according to him, is most apt to make its appearance in the dry season of the year, or in the fore part of summer and fall, a view, which I may say right here, has not found any corroboration by my own observations. In Texas, it occurs in imported Northern cattle nearly always a short time after their arrival, and independent of the season of the year.

The governor advised me to make my headquarters in New Braunfels, Comal County, and gave me letters of introduction to some of his friends, influential citizens in that part of Western Texas. One of these letters, addressed to Hon. George Pfluffer, State senator, and trustee of the Texas Agricultural and Mechanical College, procured me much aid and assistance from that gentleman. Finding no cases of cattle fever in the vicinity of New Braunfels, and not being able to learn of any cases except after they had occurred and belonged to the past, I employed my time in gathering information, in becoming acquainted with the country and the people, and in making experiments. I also visited a good many places where cattle were reported to be dying, where cattle recently had died, and where I had reason to expect to obtain information or to make observations. So, for instance, learning that Mr. J. W. Hill, 8 miles from Austin, had some recent experience in regard to so-called Texas cattle fever, I visited him on the 22d and 23d of January and ascertained the following: In the winter of 1880 Mr. Hill imported from Illinois 37 head of cattle—dairy stock of various ages. The following February they all took sick, and 28 of them died in a short time. According to Mr. Hill's description they all exhibited such symptoms—great and rapidly increasing debility, bloody or red-colored urine, &c.—as are characteristic of the Southern cattle fever. Only 9 animals, at that time mostly calves, survived. These were pointed out to me in Mr. Hill's herd, and I found them hardly any superior to his native Texas stock.

Believing that so-called Texas, Spanish, or Southern cattle fever has its source in the South, and consequently in Southwestern Texas as well as elsewhere in the Gulf States, and that the cause of the disease either originates or exists in conditions peculiar to the country, which sur

round the cattle of the South on their native ranges and act upon the bovine race either directly or indirectly by their products, somewhat like certain poisons, such as nicotine, morphine, arsenic, &c., to which the animal organism becomes accustomed, if habitually taken, first in very small and then in gradually increasing quantities, till finally a comparatively large dose remains without any visible or conspicuous effect, while a moderate or comparatively small dose is apt to have fatal consequences if taken at once by an animal not accustomed to them, I considered it necessary to become familiar with everything that pertains to the peculiarities of the life which the Texas or Southern cattle are compelled to live, particularly with their means of existence, the food they eat, the water they drink, and the manner in which they are kept and handled. The experiments I made, of course, could be based only upon theories founded upon my previous experience and observations, and upon what I had been able to learn of the nature and peculiarities of the disease. One thing, if I had not known it before I soon learned to be a fact, undisputed by those who have had any experience with Texas or Southern cattle fever, viz., that native Texas cattle never contract the fever or show plain symptoms of the same as long as they remain undisturbed on their native range; that, however, if driven North at certain seasons of the year, though they themselves remain apparently healthy, they will infect their trails, pastures, watering-places, &c., and thus communicate the disease to such Northern cattle as may pass over the same road, graze on the same range, or use their watering places after them; that in Southwestern Texas, both on the cattle ranges and on farms, only such cattle contract the fever as are imported from some place further north; and that an animal affected with the Southern cattle fever never, directly or by contact, communicates the disease to other healthy cattle.

Now, all this sounds very paradoxical and contradictory to all known laws which govern the spreading of contagious diseases, and yet it is an indisputable fact. I therefore made it a special point to solve, if possible, this mystery. As Texas or Southern cattle fever presents a great many characteristics of a disease caused by pathogenic schizophytes, and as schizophytes of the genus *bacillus* are present, have been and can be found in the morbid products and morbidly affected tissues of cattle that have died of the Southern fever, I concluded that the latter very likely belongs to that class of diseases which are caused and communicated by small microscopic organisms of the genera *bacillus*, *micrococcus*, &c. Upon this theory I based my experiments, and commenced by searching in the food and water of the cattle on the ranches for small organisms (*bacilli*) similar or identical in appearance to those I found in the morbid products and morbidly affected tissues of the animals that succumbed to the fever. Further, being acquainted with the discoveries of Dr. Hans Buchner, of Munich, who by repeated cultivations succeeded in converting innocent *acillus B subtilis* of hay-infusion into malignant

Bacillus anthracis, and *vice versa*, I thought something similar might be possible with the *bacillus* found in the cattle fever, and reasoned that if such should prove to be the case, it might not be difficult to reconcile all apparent contradictions and solve the mystery. At any rate I considered it worth trying.

Much of the land in Texas used for grazing is what is known as hog-wallow land, or land full of small, low places (concavities) a rod or even less in diameter, which in a rainy season become more or less filled with water, and constitute miniature ponds. Learning that northern cattle imported into Texas contract the cattle fever soonest, and in its most malignant form, if allowed to graze on such hog-wallow land, or to drink the water collected in those miniature ponds, I paid special attention to that kind of land and its productions. The principal grass growing on such land consists of several varieties of what is known in Texas as mesquit grass. A Texan discriminates between running, short, long, and curly mesquit grass. The latter, it seems to me, is very similar, and probably identical to what is called buffalo grass in the West, and the running mesquit, too, is not uncommon in Southern Kansas. In the hog-wallows themselves, which always contain more moisture than the ground immediately surrounding them, the grass generally is coarser and more succulent. As in Southern Texas it hardly ever freezes enough to kill vegetation, and as the climate is, on the whole, a very dry one, the grass towards winter only ripens and dries and loses its green color, but does not completely wither as it does in the north. Much of it, therefore, is really uncut hay, or converted into hay, somewhat withered, of course, and overripe while on the ground. As last winter (1882) was rather wet, the hog-wallows, at least in the vicinity of New Braunfels, were often filled with water, which not only had washed the dried grass on the surrounding higher ground, but also had carried with it from the higher places into the hog-wallows a good many grass-leaves and other vegetable substances, which, under the influence of the warm weather of a Texas spring, soon commenced to decay. Further, as the water in those hog-wallows, being very convenient, was often used by the cattle for drinking, it was necessary that I should pay special attention to and thoroughly examine it. When I put it under the microscope I found it contained, besides other minute organic forms, numerous *bacilli*, very similar in appearance, if not identical, to those which occur in the morbid products and morbidly affected tissues of cattle that die of the cattle fever. I then took some of the dry and somewhat withered grass grown near, but not in, the hog-wallows, and made an infusion with rain water. In 24 to 48 hours I had an abundance of the same *bacilli*, notwithstanding the infusion was kept in a closed vessel outdoors at the same temperature as that of the atmosphere. When repeating the experiment I had the same result; hence I concluded to repeat the experiment of Dr. Hans Buchner, and to cultivate my mesquit *bacilli* for several generations in fresh ox blood, and

if then I should meet with the same result—that is, succeed in getting in each cultivation the same or similar forms—to inoculate, after a number of cultivations considered as sufficient, some healthy animals.

Fearing, however, that inoculating Texas cattle, which, as is well known, possess immunity against the fever, would not result in anything, I made arrangements with some of my friends in Champaign, Ill., with Prof. T. J. Burrill, Ph. D., of the Illinois Industrial University, to make the cultivation and to do the microscopic work; with a butcher, Mr. John Dallenbach, to furnish the fresh ox blood; with Hon. James R. Scott, President of the Illinois State Board of Agriculture, to procure the cattle for inoculation; and with Dr. F. W. Prentice, professor of veterinary science in the Illinois Industrial University, to make the inoculations and to watch the results. I furnished Professor Burrill with some dry mesquit grass, gathered at the same place where I obtained that for my own experiments. For reasons stated I expected much more success from Professor Burrill's experiment than from my own. First as to the latter, and the rather doubtful success I met with. On January 26 I collected some dry mesquit grass in an old cattle pasture and made the next day an infusion with cold rain water. On January 29 the first examination was made, and the infusion was found to be swarming with *bacilli*, somewhat larger and thicker than the common *Bacillus subtilis*, and similar, if not identical in appearance to those I found before in animals that died of Texas or Southern cattle fever. On the 30th and 31st of January other examinations were made, but at the latter I found the *bacilli* greatly diminished in numbers. At each examination I found, also, besides the *bacilli*, some other schizomyces—bacteria and micrococci—and some puccinia and other fungus spores. I then concluded to make a fresh collection of dry mesquit grass for another infusion, and to commence cultivating the *bacilli* in fresh ox blood, which was furnished me every other morning by a butcher. I continued these cultivations till the 27th of March, and till that date succeeded in making twenty cultures.

As New Braunfels is only a small place, and has only four or five butchers, none of them have custom enough to oblige them to kill more than two or three beeves a week. I had, therefore, some difficulty in obtaining perfectly fresh ox blood when it was needed. Still the same forms, only occasionally varying a little as to size and numbers, were invariably produced. I had intended to make at least 36 or 40 cultures before putting the cultivated *bacilli* to a practical test by using them for inoculation. But as I had to leave New Braunfels to inspect some flocks of sheep in Kinney County, and as I was afraid the cultivations would not be properly attended to during my absence, I was compelled to put the twentieth culture to a test. On March 27 I inoculated an old cow belonging to Mr. Kessler, a farmer near the city. At that time I stopped with Dr. O. R. Grube, a practicing physician, and desiring to complete, if possible, the intended number of cultures

(36 to 40), I asked Dr. Grube, who is perfectly competent and reliable, to continue the cultivations for me during my absence, and left with him all of the twentieth culture that was not used for inoculation. Dr. Grube promised to comply with my request, but when I returned my material was spoiled, had become putrid, and no cultivation had been made, because the butcher had neglected to furnish fresh blood.

The next day after my return from Kinney County, I went to Mr Kessler's place to examine the inoculated cow. She appeared to be in her usual health, and nothing could be seen at the spot in the dewlap where I made the inoculation by means of a hyperdermic syringe with about thirty minims of fresh ox blood containing the cultivated *bacilli*. In the forepart of May, however, the cow commenced to emaciate and to run down, and in the forepart of summer Mr. Kessler informed me that the animal was rapidly declining, and would surely die. As she never showed any plain or characteristic symptoms of Texas or Southern cattle fever, I cannot say whether this decline was due to the inoculation or to other causes. I intended to decide this by other experiments of the same character, but as I had at that time some other work to do which required my presence at the sheep ranches, thus preventing as close and steady attention to such experiments as is necessary, and as meanwhile all the grass had become green, I thought the proper season had probably passed, and the intended experiments, therefore, were delayed till some other time.

As to Prof. T. J. Burrill's success, or rather failure, the same is best stated by giving extracts from the letters he wrote me. He writes under date of February 12, as follows :

The first attempt at cultivating your *bacillus* was a failure. On February 7, 11 a. m., I made infusions of the Texas grass and of some timothy hay taken from my stable. These were put in jelly-glasses, having a pretty close-fitting glass top or cover, and set in my laboratory, where the temperature ranged near 70° F., sometimes considerably above. I examined both each day, and found in both very few organisms, which I supposed to be *Bacillus subtilis*, but swarms of *Bacterium termo* and many *Spirilla*, and another *Bacterium* larger than *B. termo*. Supposing those worthless, I, February 10, 5 p. m., made other infusions in glass-stoppered bottles, taking care in filling these, and set them in a basement room where the temperature is about 60° F. To-day, 2 p. m., I examined these bottles, and found the liquid perfectly clear to the eye, but on examining with the microscope found a very few—one in half a dozen fields—*Bacillus subtilis* (?) in the one containing Texas grass; none in the other. One was very clear and transparent, but had a bright, oval, sharply-defined granule about the third of the way from one end. Its movement was like that of others, a slow, endwise progression, with, however, various oscillations. In both bottles were also somewhat numerous bacteria, occurring in chains or placed side by side, with little or no motion. Blood has been coming from Dallenbach's in good order, but I have not tried to use any yet. Hope these second infusions will be successful. If not, however, I will try again, changing the conditions in some way.

Under date of February 14 Professor Burrill writes :

Please send me another package of grass for infusion. I have in second lot a peculiar organism, but do not think it is the one you want; have, however, transferred to blood to try. None of the longest forms to be found now in first infusion. Please tell me how you proceeded with the infusion, whether much or little water, vessel closed or open, temperature, &c.

He writes again, March 7:

I do not make any progress with the *bacillus* cultivation. Your box of material came to hand, and has been tried in several ways, but other organisms so multiply as to completely supersede or exterminate the few *bacilli* developed. I have had no hint from you as to your method, and fear some post-office blunder.

Whether the transportation of the grass—1,200 miles—became fatal to the development of those *bacilli*, which I found in great numbers in every infusion, whether the few *bacilli* obtained by Professor Burrill were identical to those in my infusions, or whether they were simply specimens of the common hay *bacillus* (*B. subtilis*), I am not able to decide.

In my efforts to find cases of Texas or Southern cattle fever, I experienced a great many disappointments. Often cattle were reported as affected and dying with "murrain," and when I investigated the matter I found them suffering with either so-called "black-leg" or some sporadic disease. I often went where "murrain" was reported as prevailing, because the term "murrain" is used in Texas and in some other States as a convenient name for every acute and fatal disease of cattle and sheep, and which the owner of the animals is unable to diagnose, consequently Texas or Southern cattle fever also often passes under the (collective) name of "murrain," though more frequently under the name of "bloody murrain." (Cf. Governor Roberts's views, given above.) Of my frequent disappointments I will only mention one case as an illustration. On the 31st of March I received information that cattle were dying at a fearful rate at Seguin, Guadalupe County. I went there on the 1st of April, and found that 23 head of cattle, mostly town cows, had died from arsenic poisoning. In Texas it is difficult to obtain timely and reliable information concerning diseases of live stock, because, with a few exceptions, the stock men, when losing an animal, very seldom notice the symptoms, and never make a *post-mortem* examination. If an animal has died it is dead, and the rest is left to the buzzards and coyotes, which do not reveal what they find.

On May 9 I received information from Columbus, Colorado County, that of three recently imported Jersey heifers one had died of the Southern fever, and that another one was sick. As my information came from a source considered very reliable, I went at once, and returned next day. When I arrived in Columbus I found that the three Jersey heifers, or rather calves, had been imported by the assistant postmaster, one for himself and two for other parties. One of these calves had died a week or two before. All that I was able to learn as to the cause of its death was that the calf, which was kept in a yard, one evening broke out and ran off, and as it was getting dark could not be found till next morning, when it was found badly bloated (tympanitic) in the brush near the city. It was returned to its yard, but during the day it died. Consequently there is hardly any doubt that it died of tympanitis, and not of Southern cattle fever. Still, being there, I carefully

examined the other two calves, but could not find anything abnormal, except that the temperature of one, taken in the rectum, was 104°·4 F. It was, however, a very hot day, and, besides, the calf was not easily caught, and very much objected to being held, and was, in consequence, considerably excited when the temperature was taken. I afterwards corresponded with the owners of the two calves, and thus learned that both remained free from the cattle fever. In this connection it must be stated that neither one of them was allowed to run out and to graze; that both were kept, and had been kept since their arrival from the North, in an inclosed yard in which they were fed by hand and watered from a well.

In June about 150 head of cattle died within a short time in the vicinity of the Texas Agricultural and Mechanical College, near Bryan, Brazos County, but the cause of death proved to be so-called "black-leg," a disease exceedingly frequent in many parts of Texas, and very destructive to young cattle, especially in the spring and fore part of summer.

About the last of June or first of July I learned that Northern cattle had been imported and had commenced to die in San Patricio County, not far from Corpus Christi. I went there as soon as possible, and after visiting some sheep ranches in Duval County, arrived at Corpus Christi on July 5. It was my intention to go to San Patricio County, and to see the cattle there, but in Corpus Christi some other business awaited me. A very destructive epizootic which I was ordered to investigate, had broken out among the horses. So I did not go to San Patricio. Still, not much was lost by not going, for while in Corpus Christi I learned that the cattle in question had stopped dying, that about 30 head in all had died, and that the rest—comparatively few animals—had either recovered or were in a state of convalescence. The cattle were imported by Mr. Th. H. Matthews, of Rockport, and Mr. S. G. Borden, of Sharpsburg, in San Patricio County. While at Corpus Christi I became acquainted with Mr. Rogers, who lives in that place, but owns a large cattle ranch of 64,000 acres 12 miles west. He invited me to go with him to his ranch. When through with my work in town I accepted his invitation, partly to see some more of the horse-disease which extended to Roger's ranch, but principally to have a good opportunity of observing the management and treatment of large herds of cattle. While there, on July 10 and 11, I collected some dry mesquit-grass on the borders of a so-called swale, for the purpose of using it for an experiment to be related hereafter.

In the latter part of July a case of Southern cattle-fever occurred in the city of Saint Louis, Mo. Mr. Edward Morrison, of the firm of I. B. Bennett & Co., lost a grade Jersey cow. According to Mr. Morrison, the cow, before she took sick, often grazed on certain vacant lots, and cattle coming in on the cars were also frequently driven out on the same vacant lots; at least such was the case in former years, and probably also

this year. Whether Texas or other Southern cattle have been grazing on these lots this summer and spring (1882) Mr. Morrison does not know, because he was, most of the time, away from home, but does not doubt but such was the case. Notwithstanding no *post-mortem* examination was made of Mr. Morrison's cow, there can be no doubt as to the correctness of the diagnosis. Without being asked any leading questions, Mr. Morrison described the symptoms to me as follows: On a Saturday night, in the latter part of July, the cow took sick. The first symptom noticed was that she had no milk. On Sunday morning the animal was worse; her head and ears drooped, and no milk could be got. She had become very weak, was hardly able to walk, and had to be forced into the cow-yard. At noon her weakness had increased to such an extent that she had to be almost carried into the stable. A veterinary practitioner who was called gave her half an ounce of oil of turpentine and 18 ounces of sweet-oil. In the evening the animal was down, and although she made several attempts to get up, she did not succeed. As it was expected she would soon die, she was pulled out into a vacant lot, and received, at 8 p. m., one pound of sulphate of soda and two ounces of ground ginger. At 10 p. m. she made another attempt to rise, and succeeded in getting on her hind legs, but could not get up forward. When assisted and lifted up on her fore feet she was not able to stand, but rested first on her knees and then on her brisket. In about 20 minutes she was pulled down on her side to rest easier. At 12.30 p. m. she made another attempt to rise, with the same success as before, but came down again in a few minutes, and soon died. During her sickness she passed bloody or red-colored urine, and had one passage from the bowels. I made some inquiries as to other cases, but failed to get any definite information.

The next experience with Texas or Southern cattle fever, though only a slight one, I had in Champaign, Ill. While in Fort Worth, Tex., I learned, September 5, that Texas cattle fever had broken out in Wheeler County, in the Panhandle, 200 miles from the nearest railroad in Texas, and only a little nearer to Dodge City, Kan.; consequently in a locality which could not be reached in less than a week, and only at great expense. Therefore, as the information, when I received it, was at least a week old, I did not deem it advisable to go there.

On September 8, I saw in the Saint Louis Globe-Democrat, a copy of which, dated September 6, accidentally fell into my hands, that the cattle fever had broken out in Champaign, Ill., the place where I live when at home. It was a telegraphic dispatch to that paper, dated Champaign, Ill., September 5, which said that an importation of Texas fever in Champaign had caused considerable excitement; that several cattle had died, and many more were expected to die. Knowing that such dispatches are sometimes more sensational than strictly reliable, but unwilling to lose any time, and anxious to meet with cases of Texas or Southern cattle fever, I at once telegraphed to the president of the

Illinois State Board of Agriculture, Hon. James R. Scott, at Champaign—

See in Globe-Democrat Texas fever is prevailing in Champaign. If an error, answer at once; if not, shall leave here to-night.

Not receiving an answer, I left on the night train for Champaign, where I arrived on the morning of September 11. I immediately endeavored to find the alleged cases of Texas cattle fever, but was only able to learn that several town cows had died. When investigating every case I could hear of, I found that two of them had been in a corn-field, and probably gorged themselves with green corn; that one—so I was informed—had died for want of water; that one had been wounded with a pitchfork; that one had died of constipation, &c.

The rumor that Texas cattle fever was existing in Champaign had been started by a young man practicing veterinary medicine who had treated a sick cow belonging to a Mrs Price. The cow died two weeks before my arrival. A *post-mortem* examination was made by him, and he pronounced the disease Texas cattle fever. When questioned, the only thing of importance I could elicit was that the urine-bladder had contained blood and serum(?). If the bladder contained bloody or red-colored urine, his diagnosis was probably correct, because in the neighborhood of Champaign Texas or Southern cattle fever is so far the only disease of cattle which, to my knowledge, is attended with bloody or red-colored urine. Still, as this gentleman could not tell whether the contents of the bladder contained an admixture of real blood, coagulated or in streaks, as would be the case in hemorrhage in the bladder or other parts of the urinary apparatus, or whether the same consisted of urine colored red by containing in solution the red-colored constituents of dissolved blood, it could not be decided whether the case in question was Texas cattle fever or not. Mrs. Buckingham, a neighbor of Mrs. Price, had a sick cow, which recovered. The only prominent symptom observed was constipation; a passage of bloody or red-colored urine (hæmaturia) was not observed. Mrs. Fluegel's cow died, according to a neighbor, after she had been sick over four weeks; consequently, did not die of Texas cattle fever. Mrs. Cole's cow, said to be sick, was found to be not ailing at all. Mr. Spencer's cow had been sick, but was recovering; constipation, I was informed, had been the main feature of her disease. The only two cases which probably were Texas or Southern cattle fever are the following:

1. A five-year old milch cow, belonging to Mrs. Keagan, took sick on Monday, three weeks before my arrival, and died the next Wednesday morning. The cow, while sick, had hæmaturia (passed bloody or red-colored urine), became dry, fell away quite rapidly, grew very weak, and only now and then picked a little food till she died. She did not seem to be thirsty, and only once, on Tuesday night, or the night before she died, took a bucketful of water. This cow, like most other town cows, had been running out on the prairie and on the vacant lots in

the outskirts of the city. Her hide, I was informed, was covered with ticks while she was sick. These ticks, Mrs. Keagan says, may have been there before, but were not noticed.

2. Fritz Eichelberg's cow. Mr. Eichelberg is a neighbor of Mrs. Keagan, and lives nearly opposite, on the same street. The cow took sick one day later than Mrs. Keagan's, and died on Sunday, August 25. She, too, had hæmaturia (passed red-colored urine). At the *post-mortem* examination, which was made by Mr. Eichelberg himself, some blood was noticed along the back beneath the skin; the liver was noticed to be large, but was not further examined; neither were the spleen, kidneys, and urine-bladder. Mr. Eichelberg, paying his sole attention to the digestive canal, found several nails in the second stomach, but did not see any morbid changes in the organs of the chest, lungs, and heart—in my opinion, sufficient proof that the nails did not kill the animal—and had not yet done much damage, because if they had caused mischief or produced morbid changes the latter would have been in the lungs, the pericardium, and the heart, and would have been too conspicuous and too extensive to be overlooked by any one.

Mr. Barber, a neighbor of both Mrs. Keagan and Mr. Eichelberg, also had, at about the same time, a sick cow which showed symptoms of constipation and loss of milk, but had recovered when I called. Mrs. Lilly, who lives in the extreme western part of Champaign City—all the other parties, with the exception of Mrs. Fluegel, who lives in Urbana, reside on or near Springfield avenue, a street, or rather road, in the southern part of the city—also lost two cows in the latter part of August. All I could learn about them was that they died under convulsions, for the symptoms presented by the animals while sick were not observed, and a *post-mortem* examination was not made. Both of them died after a sickness of three days' duration.

During the summer, so I was told by reliable parties, one of the butchers in Champaign repeatedly bought his beeves, mostly southern (Cherokee and Choctaw) cattle, in the Saint Louis stockyards, and permitted them when unloaded at the I., B. and W. depot at Champaign, to graze on the highways and vacant lots in the outskirts of the city. Mr. Chester, who is one of the most intelligent farmers in the vicinity of Champaign, told me that in the latter part of June he met on the western portion of Springfield avenue, which, as already mentioned, is a road running east and west through the southern part of the city, quite a large drove of what he took to be Cherokee cattle, which were in charge of a herder and permitted to graze on the somewhat grassy highway. As the cattle were not far from Mr. Chester's farm, and opposite his neighbor's (Mr. J. G. Clark's) pasture, in which several very valuable thoroughbred short-horns were grazing, Mr. Chester went up to the herder and caused him to drive the cattle back. When turned and driven back some of the cattle jumped Mr. Clark's fence into his pasture, but they were at once driven out before they had time to graze.

As far as I could learn no damage was done. Whether this drove of Cherokee cattle belonged to the butcher who bought his beeves in Saint Louis or to some other party, Mr. Chester did not learn; the herder was a stranger to him. When Mr. Chester caused the drove of cattle to be turned and driven back they were near the intersection of Springfield avenue with the street on which Mrs. Lilly is living. Both streets being in the outskirts of the city, and, with the exception of the middle, partially covered with grass, are much frequented by the town cows, not only in going to and coming from their pasture-grounds, but also to graze on them. If the cows that took sick and died really had the Texas or Southern cattle fever, and as to some of them there is but little doubt, it appears probable that the drove of Cherokee cattle just mentioned caused the infection, because nearly all the parties who lost cows live in close proximity to Springfield avenue, and their animals are in the habit of passing over and grazing on that highway.

At first the evident leniency of the disease—not over 50 or 60 percent. of the animals probably affected died—made me doubt more than anything else that it was southern cattle fever, because in my former experience I found the Southern fever, if occurring in the North, an almost absolutely fatal disease; but my former experience was with cases brought on by Texas cattle coming from parts much farther south than the Indian Territory, or that portion of it occupied by the Cherokee Nation. Later observations in Southern Kansas convinced me that the fever, if communicated to Northern cattle by animals from the Indian Territory or from parts north of Texas, is seldom as absolutely fatal as that brought on by cattle from places further south. Consequently, every thing taken into consideration, I must say I am pretty well convinced that some of the town cows, for instance, those of Mrs. Keagan and Mr. Eichelberg, perhaps also those of Mrs. Price and Mrs. Lilley, and possibly three or four others, were affected with Texas or Southern cattle fever. While in Champaign I undertook some experiments, which will be related below.

Expecting that in the fall of the year I would find numerous cases of Southern cattle fever, if anywhere, in Southern Kansas, at or near the great shipping points of Texas cattle—Caldwell, Hunnewell, Harper, and Dodge City—I went from Champaign directly to Kansas, and first to Topeka, to call on the secretary of the Kansas State Board of Agriculture and the editors of some of the papers for information; but, with the exception of Mr. Baker, editor of the Topeka Commonwealth, none of them knew or had heard of any recent outbreak of Texas or Southern fever. Mr. Baker knew from his exchanges that cases had recently occurred in Barbour County. Looking over the register in the hotel where I stopped, I found the name of a gentleman from Wellington, in the southern part of the State, sought his acquaintance, and found him to be a well-informed attorney, senior member of the firm of Berry & Jones. Mr. Berry told me that to his certain knowledge I would find as much Texas cattle fever as I desired in the southern part of Harper

and Barbour counties, but particularly in the neighborhood of a small village by the name of Kiowa, near the State line. He advised me to go to the terminus of the railroad, to a town by the name of Harper, and make my headquarters there.

This advice I followed, and taking the first train that made connections I arrived at Harper on the evening of September 26, and soon learned that I would find numerous cases of cattle fever in the herds of Messrs. Bolan, Hitch & Co., near the boundary line between Kansas and Indian Territory, and a few miles east of Kiowa. I went there the next day, and found what I expected. Mr. Bolan informed me that the disease had been communicated to their cattle by a herd of Choctaws, numbering about 800 head, which in the middle of July had been grazing two weeks on their range. Their first losses, according to Mr. Bolan, occurred in the last week in August, and from that time till the 25th of September some animals had died every day, and in all some eighty or ninety of their cattle had succumbed to the disease. Only two animals, Mr. Bolan stated, had recovered; three others, yet sick, he considered as convalescent, and did not think that any deaths had occurred after the 25th. Some time later I received a letter from Mr. Hitch, in which he stated that they had lost 45 head of cattle after I had been at their ranch. So their whole loss amounted to at least 125 head of cattle. In driving over the range I saw several cattle with Bolan, Hitch & Co.'s brand evidently affected, but as none could be found that had died within the last six to twelve hours, and as the owners were busily engaged in branding calves and not inclined to sacrifice a sick animal, I had no opportunity to make a *post mortem* examination. One animal that was said to have been very sick a day or two since, and which Mr. Bolan thought might be dead and be a good subject for a *post mortem* examination, was found to be convalescent.

On making inquiries I learned that the cattle fever was not confined to the herd of Bolan, Hitch & Co., but had made its appearance wherever the Choctaw herd of cattle mentioned above had been driven or grazed, and that particularly Mr. Chawn, Mr. Campbell, Pryor Bros., and several others had lost and were still losing a good many cattle. Mr. Chawn's ranch being the nearest, and only ten miles distant, I stopped over night at Kiowa, visited the ranch of Bolan, Hitch & Co. once more in the morning, and from there went directly to Mr. Chawn's. When I arrived there I was informed that about the first of August a small drove of Choctaw cattle, constituting a portion of the herd of 800 head which had been grazing on Messrs. Bolan, Hitch & Co.'s range, were driven over Mr. Chawn's range and branded (rebranded) in his corral. The first death caused by the cattle fever in Mr. Chawn's herd took place on September 22, after the animal had been affected nearly a week, or about six days. Since then two more had died. One large bull had been very sick and was expected to die the day before my arrival, but when Mr. Chawn and myself hunted him up and approached him he

showed his horns and wanted to fight; consequently we had to consider him as convalescent. According to the observation of myself and others, it is usually a good sign or an indication of convalescence if an animal affected with Texas or Southern cattle-fever, though otherwise not vicious, commences to show fight or attempts to attack approaching persons.

When Mr. Chawn, one of his sons, and myself rode over the range, we found two other animals evidently affected, but not yet in an advanced stage of the disease. A sick two-year old steer had been seen the day before by Mr. Chawn and one of his sons about half a mile from the house; but, although we rode in nearly every direction over the range, we could not find him, till finally some buzzards were noticed flying up and lighting down again on a certain spot; and there, nearly hidden from view by the tall grass, we found our steer lying dead. The animal evidently had not been dead over an hour, for the carcass was yet warm and not damaged by the buzzards, which were just commencing their work and had only picked out one eye. The carcass, therefore, afforded an excellent subject for a *post-mortem* examination, which was at once proceeded with. When the skin was removed the flesh appeared to be almost destitute of blood, and would have resembled that of a butchered animal if the fat—the animal had been in very good flesh—had not presented a peculiar yellow (chrome-yellow) color, and if the capillaries on the lower surface of the abdomen had not exhibited a somewhat injected appearance. The blood itself was dark-colored and fluid (not coagulated), but the blood-vessels, even the large veins not excepted, contained very little of it. Internally all the fat, particularly in the abdominal cavity, presented the same deep chrome-yellow color as that on the surface of the carcass. In the chest all organs were found to be in a normal or healthy condition, but in the abdominal cavity the liver was very much enlarged (swelled), yellowish in color, and yielded, as if rotten, to the pressure of a finger; the gall-bladder was well filled with yellow, greenish-brown gall, presenting that peculiar grumous and semi-fluid condition characteristic of the disease, caused, it seems, by the immense quantity of broken-down blood-corpuscles, liver-cells, and epithelium which it contained; the spleen, too, was much enlarged and externally of a yellow and brownish or purplish-black marbled, rather pretty-looking appearance. Its parenchyma was much softened, and cutting into it its purplish-black and semi-fluid pulp oozed out. The kidneys presented externally an almost normal appearance; but on closer inspection it could be seen that their whole tissue was considerably loosened and swelled. The urine-bladder was full to its utmost capacity of red-colored urine. There was impaction of the third stomach, and a few slight erosions (loss of substance) in the pyloric portion of the mucous membrane of the fourth. Some blood and portions of liver and spleen were preserved for microscopic examination.

Southern cattle fever at that time caused losses in more than one

place in Southern Kansas; so, for instance, a Mr. Sloan, south of Elk City, in Montgomery County, it was reported to me, lost between 40 and 50 head out of a herd of 359; but as I expected to find sufficient material in Harper County—at Mr. Chawn's ranch, 22 miles from Harper, and at the ranches of his neighbors, Mr. Campbell and Pryor Bros., only 10 miles further southeast, or 30 to 32 miles from Harper—I concluded to remain, for the time being, at the latter place, especially as the cattle fever had only recently made its appearance in the large herds of Mr. Campbell and of Pryor Bros.

I must here mention that I am under obligations for favors shown me by several gentlemen—most of them experienced cattle-men—with whom I became acquainted in Harper. They took interest in my investigation, facilitated my work in every possible way, and furnished me much valuable information. Some of them went with me on my expeditions, showed me the way, to the various cattle-ranches, introduced me to the ranchmen, and assisted me in gathering information. I am under special obligations to Mr. J. McCoy, a cattle-dealer of Topeka, and formerly of Abilene, Kansas; to Mr. Elmore, of the cattle firm of Moore & Elmore, in Barbour County; to Messrs. Ewell and Sherlock, members of the Eagle Chief Pool in the Indian Territory, and to Mr. Parsons, banker, and Mr. Richards, editor, in Harper. Mr. Elmore was with me on my first trip to the ranches of Messrs. Bolan, Hitch & Co. and that of Mr. Chawn, and Mr. McCoy went with me on my second expedition, on October 2, to the ranches of Mr. Chawn, Mr. Campbell, and Pryor Bros. Mr. Chawn, up to October 2, had lost only one more animal. The bull considered as convalescent at my first visit was steadily improving and fairly out of danger.

Among the evidently diseased animals seen on Mr. Chawn's range on September 28, was an aged cow, which did not belong to the herd. When I asked Mr. Chawn about her I was told that the owner had taken her away and sold her to a butcher in Anthony. Comment will not be necessary; but I may just mention that some time later I saw in Dodge City a dressed beef hanging in the shambles that looked very suspicious, in so far as all the fat presented a very intense chrome-yellow color. From Mr. Chawn's we went to Mr. Campbell's ranch, 10 miles southeast or further below on the Little Sandy. Mr. Campbell's range is a large one and is divided in a western or upper and an eastern or lower division. When driving over the western division, where comparatively few cattle seemed to be grazing, no diseased animals were seen, but coming to the eastern division we saw some carcasses of dead cattle, and also several animals evidently affected with southern fever, though none of them in a very advanced stage of the disease.

As we arrived at Mr. Campbell's camp in the evening, we had to delay our search for diseased and dead cattle till next morning, when some of the cowboys went with us. We found first a large Hereford cow lying dead which had not been known to be sick, but as she was

in a state of decomposition and had been dead over twenty-four hours, no *post-mortem* examination was attempted. Soon after we came across a very sick four-year-old Texas cow, an animal which had been two years on Mr. Campbell's range, and had come from Texas as a two-year-old heifer. She was down, trembling all over, and evidently unable to rise; strings of saliva ran from her mouth and water from her eyes; her visible mucous membranes appeared very pale, with a yellowish tinge; the surface of her body felt cold to the touch, and her temperature, taken in the rectum, was only 96° F. Some red-colored urine was involuntarily passed, drop by drop. The animal obviously was fast sinking and beyond recovery, and undoubtedly would have died in an hour or two. Still, as I did not wish to spend much time in waiting, and desired to have my subject for a *post-mortem* examination as fresh and free from *post-mortem* changes as possible, it was decided to kill the animal by shooting her in the head and by opening the carotids (arteries on the neck). The blood from the opened arteries was thin and rather pale, and scarcely colored the hands. As soon as life had expired, the skin was removed and the examination proceeded with. The flesh under the skin looked pale, but all the fat—the carcass was in a very good condition as to flesh, and the animal must have been very fat before she was taken sick—was very yellow, though not so intensely chrome-yellow as that of Mr. Chawn's steer. The lungs, otherwise perfectly healthy, presented on their external surface some yellow spots, and their interlobular tissue appeared to be slightly infiltrated with yellow serum. The heart was in a normal condition, and healthy. In the abdominal cavity the liver was found to be considerably enlarged, yellowish-colored, and fragile; the very large gall-bladder contained the same thick or semi-fluid, grumous-looking, and viscous gall always found in Texas or southern cattle fever; and the adjoining portion of the duodenum (anterior portion of small intestine) contained in its mucous membrane numerous small extravasations of blood. The food in the third stomach was dry (impacted), and that in the fourth in an almost liquid condition. The mucous membrane of the fourth stomach, particularly towards the pylorus, contained irregularly shaped spots about the size of a five-cent piece, at which the membrane, partially destroyed, presented a dark-colored or rather blackish appearance, caused by an infiltration with extravasated blood. The spleen was very large and heavy—it weighed precisely 4 pounds—and presented on its surface a yellow and blackish-gray marbled appearance. The blood in the portal vein (*vena portarum*) was very dark, almost black. The kidneys presented an abnormally dark color, outside as well as inside, and their tissue appeared to be considerably loosened and swelled (see Plate IV). The uterus, which contained a small fœtus, about seven inches long, presented numerous ecchymoses on its interior surface. The urine-bladder was much expanded with deep red-colored urine. No other morbid changes. Afterwards we found another dead

animal at or near Mr. Biddle's range, which adjoins Mr. Campbell's to the northeast.

The carcass was very much decomposed, and, therefore, no *post-mortem* examination was made. As Mr. Campbell was not at his ranch, I did not learn the exact number of cattle he had lost. His men estimated his losses at from 25 to 40 head, and the loss of his neighbors toward the south, or over the line in the Indian Territory, Pryor Brothers, was stated to be between 40 and 50 head. Other neighbors toward the north and east also lost several animals.

The same herd of Choctaw cattle, or at least a portion of that herd which was accused of infecting Messrs. Bolan, Hitch & Co.'s range, and Mr. Chawn's range, was also accused of infecting the ranges of Mr. Campbell and his neighbors. Although the accusation, I have no doubt, is well founded, the herd of Choctaw cattle, it seems, was not the only drove of Southern cattle that passed the State line, because some of the cowboys told me that another drove of Southern cattle, not belonging to the Choctaw herd, had crossed within three weeks. What became of them I have not been able to learn. On my return to Harper I was informed that some cattle belonging to Dr. McManigle, in Harper, and recently imported from Missouri, had commenced to die of Southern cattle fever. I immediately called on Dr. McManigle, and learned that of 27 head of so-called stockers, which had been bought in Missouri, and had recently arrived in Harper over the Kansas City, Lawrence and Southern Kansas Railroad, one steer was dead, and two cows diseased. As the Kansas City, Lawrence and Southern Kansas Railroad transports a good many Texas and Indian Territory cattle to the Eastern markets, it was supposed that Dr. McManigle's cattle had been shipped in an infected car, and in that way contracted the disease. Such, however, as was afterwards proved, was not the case.

On the morning of October 4 the doctor and myself went out to his range, which is only a few miles from Harper. His cattle, which, he informed me, had been bought from different parties in Missouri, had been shipped on board of cars on September 11; consequently they had been on his range only a little over two weeks. First, one steer was taken sick, probably on September 28, and it was found dead on October 2. Then, on September 30, two cows were noticed to be affected at the same time. When we arrived at the range we found one of the cows, a white and red animal, lying down in a ravine, and hardly able to rise, but at last, after making several attempts, she succeeded in getting on her feet, and then, although hardly able to walk, she showed her horns and endeavored to attack us. Being unprepared for such an emergency, and having no ropes, we could not make any close examination. The other sick animal, a red cow, in good condition as to flesh, was found lying down on the brow of a hill; she, too, could only get upon her feet with great difficulty, and after making several attempts. When she stood on her feet she trembled all over, slavered

very much, chewed her saliva, was hardly able to stand, reeled to and fro, placed her feet in an awkward position, the fore feet rather backward, and the hind feet too far forward. Her ears were drooping, and her nose was most of the time held near the ground. Her excrement, which was voided in my presence, contained an admixture of blood and a considerable quantity of gall. Her temperature, taken in the rectum, was only 102° F. Expecting that one of the cows, if not both, might possibly die towards evening, and desiring to have my subjects for *post mortem* as fresh as possible, I induced Dr. McManigle to go again with me to his ranch in the evening. When we returned, the first (white and red) cow, which showed fight in the morning, could no where be found; even a two hours' search along the ravine and in the long grass proved fruitless. The red cow, which in the morning was found at the brow of a hill, was at nearly the same place, and was almost precisely in the same condition as when last seen. Her weakness, however, was evidently greater than in the morning, and judging from that, combined with her low temperature, I expected to find her dead, at any rate, by next morning. Dr. McManigle thought different. Somebody had told him of belladonna being a sure cure for Texas cattle fever, and he had some *extractum belladonnæ*, enough for two doses with him, and wanted to try it. He therefore gave one dose to the red cow at about 4 p. m., and when we had to give up our search for the other cow on account of darkness setting in, he gave her the other dose intended for the white and red cow.

Next morning, October 5, we made another visit, and found the red cow dead at nearly the same place we had left her the previous evening. She probably died early in the morning, and when we arrived, at 8 a. m., had been dead at least three hours. I at once proceeded to make a *post-mortem* examination. When the skin was removed the flesh, with the exception of a few small places which had been bruised, presented a pale and bloodless appearance, and the fat, as is often the case in Southern cattle fever, was of a deep yellow color. On opening the chest the lungs were found to contain considerable œdema, but were otherwise healthy, and at least partially collapsed. [œdema of the lungs cannot be considered as a morbid change belonging to, or directly caused by, the morbid process of Southern cattle fever, for it is a common occurrence, and often found at *post mortem* examinations after great suffering and protracted agonies of death.] In the abdominal cavity the liver was found to be very much enlarged, and exhibited, not only on its surface but also in its tissue, a decidedly yellow tinge. The large or abnormally expanded gall-bladder was filled with a thick semi-fluid, grumous-looking, and dark greenish, brown-colored gall. The spleen, too, was much enlarged, and weighed three pounds and 9 ounces. Its surface presented a grayish black and yellow marbled appearance, and its pulp, when the spleen was cut into, was semi-fluid, oozed out, and presented a purplish-black color. The third stomach was much impacted and the contents

perfectly dry, or dry enough to be powdered; but in the fourth stomach the contents, which contained some partially digested grass, were almost liquid, and the folds of the mucous membrane presented a slate-gray color, and towards the pyloric or posterior portion of the organ some dark, blood-stained erosions and loss of substance. The kidneys presented exteriorly an almost normal appearance, but interiorly their texture appeared to be loosened and swollen. The urine bladder was nearly empty, and contained only a few drops of a dark red fluid (urine), and the uterus contained a female fœtus about ten inches long. The blood-vessels, even the large veins, such as the vena cava, anterior and posterior, as in other cases, contained very little blood, so that I had difficulty to fill, without allowing too much access of air, a small two-drachm vial with blood for microscopic examination.

When the *post-mortem* examination was finished we renewed our search for the white and red cow, and found her fully one mile distant from where she had last been seen. She was not only alive, but had considerably improved, and even made attempts to nibble a little grass. I therefore considered her as out of danger, notwithstanding she failed to get her dose of *extractum belladonnæ*. Returned to Harper I induced the butchers of that town to weigh the spleens of the animals butchered for meat. In a few days they reported an average weight of two pounds, or just about half the weight of the spleens of the diseased cattle.

It was mentioned above that facts existed showing that Dr. Manigle's cattle did not contract the disease by becoming infected in a railroad car. As stated before, the cattle were bought of different parties in Missouri, and consequently had different ear-marks. When the doctor and I had finished our *post-mortem* examination, and had found the missing cow, it struck me that the two cows and the dead steer had the same ear-marks. (I inspected the carcass of the dead steer, but did not make a *post-mortem* examination on account of advanced putrefaction.) So I suggested it might be worth while to examine the ear-marks of the remaining 24 head of cattle, which were then healthy, and have remained healthy, to see if any of them had the same ear-marks. We examined them all, and found the ear-marks of every one of them entirely different from those of the two cows and the dead steer. Consequently, it became evident that the two cows and the steer must have belonged to the same party, and the other twenty-four animals to other parties. Further, as only the three animals which belonged together and had been owned by one and the same person, and none of the others bought of different parties took the disease, and as any possibility of an infection at Dr. McManigle's range was excluded, there can hardly be any doubt that the two cows and the steer became infected with the Southern cattle fever while in possession of their former owner, and were possibly sold on that account. If, on the other hand, an infection had taken place in the railroad car—the 27 cattle were shipped in one car—it is to be supposed that all, or nearly all, would have taken the disease; at

any rate, it would be very strange that just three animals having the same ear-marks, and evidently belonging together, and none others, should take the disease.

On October 6, 7, and 8, I made another tour to the cattle ranches on the "Little Sandy," and along the boundary line between Kansas and the Indian Territory. I came first to Mr. Chawn's ranch. His losses till date among his own cattle—he also kept cattle for other parties—amounted to four head. The bull, which was recovering when I was there last, had not been seen during the last two days, but was supposed to be steadily improving. From Chawn's I went to Campbell's and Pryor Brothers' ranches. Their combined losses were estimated at 70 to 80 head. No new cases had occurred. I saw one animal in an advanced stage of cattle fever, but could not ascertain to whom it belonged. It probably belonged to one of Mr. Campbell's neighbors, a Mr. Biddle, who had also lost between 15 and 20 head of cattle. On the evening of the 6th, while at Mr. Campbell's ranch, it commenced to rain, and during the night and on the 7th it rained in torrents. In consequence, the little creeks, which I had to cross to get back to Harper, but particularly the "Little" and "Big Sandy" further north, swelled to such an extent as to become unfordable; I thus became "water bound," and could not get away till the 8th, when I returned to Harper.

On October 16, I received notice that Texas or Southern cattle fever had broken out in the herd of Messrs. Moore and Elmore, 27 miles west southwest of Harper, in Barbour County. I went at once, but accidentally losing my way, I arrived at the ranch of Mr. Tom Kelley, $1\frac{1}{2}$ mile south of Moore and Elmore's camp. Mr. Kelley has only a small herd of cattle, consisting of about 40 head; three animals out of these forty, so Mr. Kelly told me, had recently been affected with Texas or Southern cattle fever, and two of them had died and one recovered. He also informed me that one of his neighbors, a Mr. Munger, had brought in in the latter part of August a herd of Choctaw or Arkansas cattle, which had been on his (Mr. Kelley's) range and had also imparted the disease to the cattle of Messrs. Moore and Elmore. The latter I learned when I arrived at their camp had lost four head. Mr. Moore and I, when riding over the range, found one animal which had just died, lying doubled up in a deep narrow hole washed out by the rains. It must have fallen in while in the agonies of death. As it was impossible to get the carcass out of the hole without a hoisting apparatus, a *post-mortem* examination could not be made. One steer, a yearling, supposed by Mr. Moore to be dying, was found to be convalescent, at any rate was able to run fast enough to prevent us from catching him. Mr. Moore told me that his neighbor, Mr. Munger, bought in the forepart of summer a herd of cattle in the vicinity of Fort Smith, Ark., and kept them about two months in the Indian Territory, before he took them to his Kansas range, which joins the range of Messrs. Moore and Elmore. The latter moved their cattle to their present range on the "Salty," on September 27. Before

that time they had been temporarily occupied or run over by the Arkansas and Choctaw cattle of Mr. Munger, and the first case of disease among Moore and Elmore's cattle occurred on October 12, or fifteen days after the cattle were moved to the new range. On their old range, or previous to September 27, their cattle did not pass over any range where Arkansas, Choctaw, or other Southern cattle had been, consequently the period of incubation cannot have exceeded fifteen days, and must have been very short, for two weeks is about the shortest period of incubation ever observed in the West (*cf.* below). I learned afterwards that Mr. Munger's Arkansas and Choctaw cattle arrived at his Kansas range about the last day of August. I also received some further information concerning the herd of Choctaw cattle, which imparted the cattle fever in the southern part of Harper and Barbour Counties, along the State line. They were imported by Messrs. Morton and Tulliver, and bought by them 40 miles south of Fort Smith, Ark. A part of the herd, I learned, was bought in Arkansas, and a part in the Indian Territory of a Mr. Thomas Overstreet, a so-called squaw-man in the Choctaw Nation. On October 24 I met Mr. Moore, and learned that recently three more animals had died of the Southern fever, and that the yearling steer, mentioned above, was living and improving. Messrs. Moore and Elmore's whole loss, consequently, amounted to 7 head of cattle.

Major Drumm has about 13,000 head of cattle on his range in the Indian Territory, and lost about 15 head, of Texas or Southern cattle fever. The Choctaw herd of cattle, which communicated the disease to Pryor Brothers' herd and to others in Southern Kansas, and which, according to the major, contained 2,200 head of cattle—the herd must have been divided when it reached the Kansas line, because Mr. Bolan and others stated the number of cattle in the herd to be 800—was driven through a corner of the Major's range. According to Major Drumm, Pryor Brothers lost in all over 50 head, while Pryor Brothers' cow-boys would not admit that they had lost over 45. It is almost impossible to get accurate information, particularly as to the number of cattle diseased or dead, because those large herds range over a great extent of territory, and it often happens that some animals are not seen in several months, or until the next "round-up."

On October 18 I was informed by Mr. McCoy that losses by Southern cattle fever had occurred in Mr. Frances' herd, 11 or 12 miles from Harper. The distance not being great, I went there the same day and found that 6 animals had died; 8 in all had been sick, and 2 had recovered; also that the disease had been brought on by some Choctaw cattle which had trespassed on Mr. Frances' range. Whether these Choctaw cattle belonged to the same herd which caused so much damage in the southern parts of Harper and Barbour Counties, or not, I did not learn. Mr. Frances did not happen to be at home, and I had no chance to see his cattle; still, as none of them were at that time seriously affected, and as none had died within the last 24 hours, it did

not matter very much whether I saw them or not. The period of incubation had been over a month.

On the 21st of October I received a very interesting letter from Mr. Campbell, which I will give in full, especially as Mr. Campbell is a man of considerable experience, and knows what he is talking about.

CALDWELL, KANS., October 20, 1882.

Dr. H. J. DETMERS, *Harper, Kans.* :

SIR: I very much regret that I did not have the pleasure of meeting you at my ranch and witnessing your *post-mortem* examination of diseased cattle there. I have lost many valuable cattle from time to time during the past eight or ten years by Texas or Spanish fever, and though I have tried many so-called remedies, I have found none infallible. The best remedy I have ever found was a removal of "the entire herd" on to fresh grass and water that had not been used by other cattle. But this remedy of late years is absolutely impossible, as every nook and corner is now used by some one. My theory is that the disease is imparted by a deposit of slabbers or saliva from the mouth of Southern cattle upon the grass, and then imparted to the native cattle that afterwards graze thereon.

I am satisfied the disease was this year imparted to my cattle by a herd of cattle driven from Arkansas or the southeastern portion of the Indian Territory.

Tinct. extr. belladonnae is highly recommended as a remedy, but I have not yet tested its value. I returned from my ranch last night, and am glad to inform you that the disease is abating, and I hope the recent frost will effectually stop it, though I have known the disease to prevail to an alarming extent during a warm spell after two or three heavy frosts.

As you are aware, I am deeply interested in discovering the cause and cure of this dreadful disease, and if I can possibly spare the time to call on you before you leave the State I shall do so. But as I am obliged to leave for the East to look after the sale of my beeves, I fear you will have gone before my return. This being the case I beg that you write me at this point, giving your views as to the nature and cure of the disease, and whether or not you think the disease was caused by Arkansas or Southeastern Indian Territory cattle crossing over their range.

I have dissected quite a number of cattle affected with, or that have died from, Texas fever, but not possessing a medical education, I have only been able to note the most conspicuous symptoms and irregularities and jump to conclusions therefrom. Still, if I can be of any service, or render you any aid in your investigation, I will cheerfully do so.

Hoping to hear from you at your earliest convenience,

I am yours respectfully,

W. E. CAMPBELL.

It will not be necessary to make any comment on the above communication. Mr. Campbell's observations, it seems to me, are correct, and particularly his remark that Texas, Spanish, or Southern fever is not always brought to a stop by the first frost, corresponds with my own observations made this winter (1882-'83) in Texas. I also firmly believe, nay, am convinced that it is the slaver and saliva by which the infectious principle is deposited on the grass and in the water.

As to the tincture of belladonna extract, I do not think it will have any effect upon the morbid process of Southern cattle fever; much more may be expected of medicines which possess antiseptic properties, and, at the same time act upon the liver, which undoubtedly constitutes the principal seat of the morbid process. But about this below.

At this time reports of cattle dying in the vicinity of Dodge City, and on the trail from the "Panhandle" to Dodge City, came in every day; I therefore considered it my duty to go there for the purpose of investigation.

At Dodge City, where I arrived on the evening of October 24, I found that Messrs. Wright & Barry had suffered considerable loss. Mr. Wright informed me that out of a herd of 60 head of cattle they had lost 20. The dying commenced in the latter part of August, or forepart of September, when 10 animals became affected at the same time. Next day some more were found to be sick, and they, too, like the first 10, soon died. The herd was then removed to another range where no Southern cattle had been, and the dying ceased. Only one animal died after the herd had been moved, but the same was sick—showed plain symptoms of the disease—before the removal of the herd. The disease was imparted as follows: Six or seven weeks before the first cattle took sick, five or six lame Texas steers, unable to proceed on their journey with the drove to which they belonged were left behind and allowed to stay a week or ten days on the range. On October 25, I interviewed Messrs. Beeson & Herrs. They had lost between 40 and 50 head. Their cattle commenced to die on or about the first of September, and all died within two days. A herd of "through" cattle ("through cattle" means cattle coming on foot directly from Texas) had passed over the range about two or three weeks before their cattle commenced to die. To their knowledge no other Southern cattle had been there. Their whole herd numbered about 250 head, and those that died contracted the disease by drinking out of waterholes at which the Texas cattle had been watered.

Mr. Anthony, at Dodge City, lost 17 fine dairy cows out of a herd of 23. In all, 18 animals took sick, but one recovered. Some Texas cattle had been on the range about two months before Mr. Anthony's cows commenced to die. The first deaths occurred about the 1st of September, and the last ones ten days later. Mr. Anthony thinks it is the slaver of the Texans deposited on the grass and in the water that causes the infection, and constitutes the medium by which the disease is communicated. Mr. Anthony's whole herd originally consisted of thirty odd head of cattle, but the 23 mentioned were the only ones that grazed where the Texas cattle had been; the others were kept in a pasture and were not allowed to run out on the range.

As it was repeatedly asserted by stockmen in Kansas that Texas or Southern cattle fever never occurred in Colorado, I thought it worth while to go to the State line and a little beyond to ascertain the truth, and, in case it should prove to be true, the probable cause. I soon, however, convinced myself by making inquiries of Colorado cattle men that the cattle fever does occur in Colorado, though, as a rule, only in such a mild form as to cause but very few losses. The only thing by which, to my judgment, this greater leniency of the cattle fever in Col-

orado can be accounted for must be the higher altitude, and probably also the fact that the cattle in Colorado, almost exclusively, get their water for drinking from mountain streams with enough current to immediately wash away any slaver, saliva, or other vehicle of the infectious principle with which the water may happen to be contaminated.

As it commenced to freeze in Kansas, and as therefore not many more cases of cattle fever could be expected, and as also the time had arrived at which the Texas ranchmen make importations of Northern cattle, I returned to Texas. As the most direct route to Southern Texas is through Fort Worth, I stopped over one day at that place, not because I expected to find there material for investigations, but for the purpose of learning from the editors of the Texas Live Stock Journal where and by whom Northern cattle had been, or were expected to be, imported. From Fort Worth I went back to San Antonio as the most suitable place, because nearly all the Northern cattle imported in Southwestern Texas are shipped by rail to that point, and are from there driven and shipped to their places of destination.

On November 10, Major Hinkle, the representative of the Texas Live Stock Journal at San Antonio, received 22 Hereford bulls, which were shipped to him from Bucher, Ill. When these bulls, which were imported for the purpose of selling them to the cattle men, had arrived, they were kept in a stockyard on South Flores street, and fed with hay, bran, and meal, till sold and delivered. Their ages ranged from one year—the youngest—to three years past—the oldest. As early as November 13, one of these bulls showed some indisposition, manifested by a partial loss of appetite, and thinness in the flanks, or a gaunt-looking appearance. His muzzle, however, was not dry, but moist and pearly like that of a healthy animal, and it is very possible that the insufficient appetite and the gaunt appearance were simply the consequence of the hardships endured on a long journey. As the animals were for sale, constantly surrounded by a crowd, and I and my business well known, the owner did not desire me to make a close examination, and so it is difficult to decide whether the animal was only fatigued or already affected with Southern cattle fever. All the bulls were sold and delivered on November 14. Thirteen of them went to Messrs. Lytle and McDaniels in Medina County, seven to Mr. W. G. Butler, Daileyville, Karnes County, and two to Mr. H. C. Grines, Brackett, Kinney County. Messrs. Lytle and McDaniels resold one of their lot of thirteen to another party, the name of which I did not get. As in the following a detailed account will be given of the animals purchased by Messrs. Lytle and McDaniels, it may suffice to say that all others died within about three weeks, or were dead before the middle of December. I made my first visit to Messrs. Lytle and McDaniels's ranch, about 25 or 26 miles from San Antonio, on the 1st of December, and found that two of their bulls had died, one on November 27 and one on November 29. On December 1 only one of the remaining ten bulls showed plain symptoms of Southern cattle

fever, such as drooping ears, a peculiar look of the watering eyes, slow movements, a certain stiffness in the limbs, some weakness in the back, thin flanks, strings of saliva spinning from the mouth, &c. I staid over night on the ranch, and on the 2d found two more animals plainly diseased. The one that was sick the day before was evidently worse, much weaker, and thinner in the flanks, while another one, also a two-year-old bull, was about in the same condition as the first one on the day before. The third one showed only slight indications. The temperature of No. 1—for convenience sake I will designate them by numbers—was 107° F., and that of No. 2 was 106° F., while that of No. 3 was only 105° F. It may not be superfluous to state that wherever the temperature of an animal is given in this report it means the temperature in the rectum, taken by inserting a self-registering thermometer full four minutes. For comparison the temperature of an apparently yet healthy yearling—the youngest bull in the lot—was also taken, and found to be precisely 104° F. It appeared to me that every animal in the lot was already affected, or in the first stage of the disease, because none of them showed that liveliness and spirit one may expect to find in a well-fed and well-cared-for lot of young bulls in good flesh. The dung of the sick animals was rather scant, a little too hard, and coated with a layer of mucus.

Expecting to find subjects for *post-mortem* examination, I went again to Messrs. Lytle and McDaniels's ranch on December 4, and arriving in the evening staid over night. On December 5 all the bulls were sick; some of them, however, were yet in the first stages of the fever. Bull No. 1 was dead, and had died during the night; No. 2 was fast sinking, his temperature was down to 102.5° F.; No. 3 had a temperature of 107.5° F., and was very sick; No. 4, an animal past 3 years old, and the oldest, largest, and naturally most vigorous bull in the lot, also had a temperature of 107.5° F., and was sure to die. The other six bulls were either yet in the first stage of the disease or had just passed it, and were moping about with drooping ears, watering eyes, strings of saliva running from the mouth, &c. They all had lost flesh, were thin in the flanks, stiff in the limbs, and very weak, and all those that were seen making water passed red-colored urine (had hæmaturia). Only one of them, not quite so far advanced as the others, was seen nibbling a little food.

Post-mortem examination of bull No. 1: When the skin was removed the flesh (muscular tissue) looked like that of a butchered animal, and was destitute of blood. (It must here be mentioned that Mr. McDaniels, being at a loss what to do, had severely bled all those animals that proved to be very sick. No. 4, the old bull, particularly, lost a large quantity of blood and was still bleeding.) All blood-vessels, the large veins as well as the small ones, were empty, or contained but an insignificant quantity of blood. No abnormal yellow coloring of the fat was observed. The liver was very much enlarged, and very icteroid, or

impregnated with yellow; the gall-bladder was large and full of a yellow greenish-brown and thick or semi-fluid, grumous-looking gall, characteristic of the disease, and the adjoining tissues appeared to be permeated with gall. The spleen was exceedingly large; its weight was estimated to be over five pounds, and presented on its surface a yellowish-red and grayish-black marbled appearance; it was very fragile, and its purplish-black pulp oozed out on the slightest incision. The kidneys were abnormally dark colored, and their tissue loosened; and the urine bladder contained a large quantity of red or blood-colored urine. As other but less essential morbid changes may be mentioned some emphysema in the lungs and a few erosions in the pyloric portion of the mucous membrane of the fourth stomach, which latter itself contained only a small quantity of very thin feces. There was no impaction in the third stomach, and all other internal organs appeared to be healthy or in a normal condition. As the animal had died past midnight, or perhaps not till early in the morning, no decomposition had set in, and *post-mortem* changes were not present.

Bull No. 2, which in the morning was down and unable to get up, and in a semi-unconscious or comatose condition, died at 12 m., and the *post-mortem* examination was made at 1.30 p. m. When the carcass was skinned, the flesh, like that of bull No. 1, presented a nearly normal appearance, almost destitute of blood, and only the veins about the head discharged, when cut, a small quantity of a brownish fluid. The lungs contained here and there a little emphysema, undoubtedly caused by difficulty of breathing during the agonies of death, but were otherwise perfectly healthy. The liver was very large, and its tissue, permeated with gall or with yellow exudation, presented a strong yellow tinge; its actual weight was 19½ pounds. The gall bladder was large and full of a thick, viscous, yellow-greenish brown and grumous gall, the same as in No. 1. The spleen also was exceedingly large, and presented on its surface a marbled appearance; its actual weight was 6 pounds. The kidneys, too, were abnormally large, apparently swollen, their tissue considerably loosened, and the fat in the pelvis areolated or emphysematic. The urine bladder contained a quantity of pale red urine, which in the air changed its color to a pale purple. The contents of the third as well as of the fourth stomach presented a normal consistency; there was no impaction of the third stomach, and the mucous membrane of the fourth stomach contained only a few slight erosions near the pyloric opening. On the surface or outside of the body the color of the fat was normal, but in the abdominal cavity it was a distinct chrome yellow. The animal at its death was about two years old, and No. 1 was of the same age.

No. 3, also a two-year-old bull, but for his age a rather small animal, had in the afternoon a temperature of 107° F., and passed dark red-colored urine, was exceedingly weak, hardly able to stand, and his eyes

were glaring. The lower temperature, therefore, was not an indication of improvement, but rather a sign of approaching death.

No. 4, the oldest and largest bull in the lot, had in the afternoon a temperature of 106° F., consequently $1\frac{1}{2}^{\circ}$ F. lower than in the morning, and, it seemed to me, was fast sinking. Mr. McDaniels, however, ascribed the decrease of temperature to the great loss of blood; the bleeding had not entirely ceased.

It must here be remarked that the Hereford bulls of Messrs. Lytle and McDaniels had a very comfortable stable, surrounded by a yard, which stable and yard were occupied by them during the night and in the morning. The yard contained some troughs always well supplied with chopped food, bran, and meal, of which they could eat at their pleasure. During the day, however, they were running out and grazing on so-called hog-wallow land. Still, I am inclined to think the infection took place before the animals reached Messrs. Lytle and McDaniels's ranch, probably in the stock-yard in San Antonio, or perhaps while on the road when unloaded and fed and watered in a railroad cattle-yard, because the bull mentioned as showing some indisposition when in the stock-yard on South Flores street, in San Antonio, was the first one reported to be dead. He was one of the two sold to Mr. Grines, in Brackett, Kinney County.

As other cases were awaiting my attention, and as the distances in such a thinly-settled country as Southwestern Texas are very great, I was obliged to leave, but took with me the conviction that Messrs. Lytle and McDaniels, if very fortunate, would not save more than two or three animals, which at the time of my presence had not yet reached the height of the disease. I did not afterwards learn anything positive, but heard that every one of the bulls had died. All the others, those sold to Mr. Butler and to Mr. Grines, died.

On December 6 I went to Mr. Harvey Terrell's ranch, $1\frac{1}{2}$ mile from the Government (military) depot near San Antonio. Mr. Terrell received on November 25 11 Devon calves and yearlings, males and females, and one seventeen-months-old Jersey bull. They were imported from Ohio and comfortably housed on Mr. Terrell's ranch in a good, well-ventilated stable, which was left open in day-time so as to allow them to take exercise in a well-fenced yard, and to give them access to a fenced-in patch of green oats. They were watered with hydrant water coming from the San Antonio Springs, and their food consisted of corn fodder, green oats, corn-meal, bran, and scorched prickly pears. One of the calves, the youngest in the lot, and only three months old, was nursed by a Texas cow, which was twice a day admitted into the yard to suckle the calf, but was not allowed to eat and to drink there. Although nearly all the arrangements just described must be considered as unobjectionable, the possibility of an infection was not entirely excluded, and I deemed it advisable to prescribe some medicines to be used as prophylactics. When at the ranch,

Mr. Terrell's father, Colonel Terrell, chief paymaster, U. S. A., at San Antonio, was with me, and he and I agreed to divide the calves and yearlings into two or three divisions, and to give to one division once a day, for each hundred pounds of live weight, from eight to ten drops of pure (95 per cent.) carbolic acid in the water for drinking, and to the second division, for each estimated hundred pounds of live weight, one drachm of chinoidine. If three divisions, each composed of four animals, were formed, the third should receive, according to Mr. Terrell's choice, either nothing at all or both the carbolic acid and the chinoidine. The medicine required was at once ordered as soon as I returned to town, but was not called for till December 8, and probably not used till December 9. On December 9, however, every one of the twelve animals were sick, at least showed plain and unmistakable symptoms; but with the exception of three of them they all recovered. The three that died were the Jersey bull, 17 months old, and the oldest and the youngest of the Devons. The youngest, it will be remembered, was suckled by a native Texas cow. Dr. F. Terrell, another son of Colonel Terrell, and a practicing physician in San Antonio, made the *post-mortem* examinations of the three animals that died, and found the same morbid changes which I found in other cases.

Learning from Major Hinkle that Mr. J. V. Abrams, in Uvalde County, a gentleman with whom I had been acquainted, and who had shown me some favors in my investigation of sheep diseases, had imported some Illinois cattle and taken them to his ranch, I wrote to him about these cattle, and received an answer by telegraph inviting me to visit him. I immediately replied that I would come on Monday, January 7, and requested him to meet me at the depot at Uvalde Station. As it happened, his dispatch was sent on Friday forenoon, but did not reach me, was not delivered till Saturday afternoon; and although I immediately replied, he did not expect another answer by telegraph, and, living eight miles from town, did not get it. Consequently when I went to Uvalde, on the 7th, I did not find him at the depot, and not knowing where he kept his cattle, or whether he had them at his house, 8 miles west of Uvalde, or at his ranch, 12 miles north of that town, I went to his residence, where I was informed that he kept his cattle at his ranch, and that he too had gone there in the forenoon. So I had to return to Uvalde to stay over night. Next morning I procured a team and drove out to his ranch, but when I arrived there Mr. Abrams had just left for home by another road; still, although I missed him, I saw his cattle, fourteen heifers and two bulls, and found some of them—two or three heifers—affected with the fever, but yet in the first, or, as I would like to call it, moping stage of the disease.

The animals drooped their ears, appeared to be somewhat thin in the flanks, moved less lively than a healthy animal, and did not seem to care much for their food. Most of the cattle, however, appeared to be healthy, at least did not show any symptoms of disease. As the owner

was not there, and nobody but a Mexican cow-boy in the camp to take care of the cattle, nothing could be done. When returning to Uvalde I missed Mr. Abrams again, for he had just gone home, but had left word that he would be in town and meet me next morning before train-time. Next morning train-time arrived, but Mr. Abrams was not there; consequently I left for San Antonio. At Hondo Station I received a telegram from Mr. Abrams asking me to return. This I could not do on account of other appointments in San Antonio, but I returned to Uvalde on January 11, and meeting Mr. Abrams at the station, went immediately to the ranch, or rather cattle-camp. Arrived there, I found several of the heifers—they were nearly all animals from 16 to 20 months old, and full-blood and half-bred short-horns—considerably affected. One of them, which was by itself in a separate pen, passed red-colored urine, and showed all other symptoms characteristic of Southern cattle fever, but her temperature was only 103.5° F., and I did not regard her as dangerously sick. She and another white heifer had been the first to show any symptoms of disease. Another one, though not in quite as far advanced a stage of the fever, was evidently much worse—had a severer attack—and was very weak. Her temperature was 105.2° F. Two other heifers, in about the same stage of the disease, had a temperature of 103.5° F. and 104.2° F., respectively. These three, and to a certain extent two or three others, showed all the characteristic symptoms of the first stage, viz., drooping of head and ears, watering of the eyes, spinning of strings of saliva from the mouth, thinness of the flanks, visible emaciation, great weakness, a staggering gait, and hæmaturia, or red-colored urine. Another heifer, a white one, which appeared to be the most affected at my first visit, and was said to have been the first one that showed any symptoms, had not yet recovered, nor much improved; still, as she was no worse, I considered her as convalescent, or at least as not being dangerously sick. Her temperature was 103.5° F. In order to make an experiment, I gave to each of the three worst heifers, which were yet in the first stage of the disease, and which for convenience sake I will designate as Nos. 2, 3, and 4, six drachms of chinoidine in solution at one dose, or about one drachm for each hundred pounds of (estimated) live weight. I chose chinoidine instead of quinine because it is so much cheaper and about as effective as the latter. As there were no accommodations whatever for staying over night at the cattle-camp, Mr. Abrams and I had to go to his sheep-camp, 4 miles distant, where he has a large tent. During the night a severe "norther" came up. In the morning we returned to the cattle-camp, and found the three heifers which received the chinoidine very much improved. No. 2, which on the evening before had a temperature of only 105.1° F., had in the morning a temperature of only 101.5° F., and looked otherwise much better. The temperature of Nos. 3 and 4 had also considerably decreased, and their general appearance had improved, while those sick heifers which had not received any chinoidine

were about the same, or at any rate no better than in the evening before. No. 1, the heifer in a separate pen, which in the evening had a temperature of 103.5° F., had a temperature of 103.6° F. We then concluded to also give a dose of chinoidine to some of the others, and to watch the result. In the evening, and still more next morning, an improvement was visible, particularly in all those that received the medicine, which of course had been given to the worst ones. The history and treatment of these 16 head of cattle, which, with the exception of two young bulls each less than a year old, were all more or less affected with cattle fever, but will, I think, all recover, can be stated as follows: They were shipped from Aurora, Ill., to Saint Louis, from Saint Louis to Cairo, from Cairo to New Orleans, from New Orleans to Houston, from Houston to San Antonio, and from San Antonio to Uvalde. At all these stations except at Uvalde they were unloaded to be fed and watered in the railroad feeding pens, and of course received only such feed and water as is common and can be obtained at those stations. Consequently the animals had abundant opportunities—at any rate in New Orleans, Houston, and San Antonio—of becoming infected with Southern cattle fever long before they reached their destination. When they arrived in Uvalde, on December 25, 1882, Mr. Abrams fed them in the car with sheaves of dried green oats (so-called oat-fodder), which he had procured in San Antonio, and then muzzled every one of them before they were unloaded and driven to his ranch or cattle-camp, so as to prevent them from eating any grass on the way. At the ranch or camp Mr. Abrams had five small pens (four for the heifers and one for the bulls) prepared and ready for their reception, and when safely in these pens their muzzles were taken off. Their food, which they received in the pens, was carried to them, and consisted exclusively of bran, oats, scorched and cut-up prickly pears (*Cactus opuntia*), and sotol, and their water for drinking, which was hauled in barrels on a wagon from a small creek, the "Yellow Banks," about two miles distant, was also carried to them into their pens. Besides, Mr. Abrams took pains to keep the bowels of his cattle open. The Mexican who had to feed them, and was constantly with them, had strict orders to notice the condition of the excrements of every animal, and if any one of them happened to show the least signs of constipation it at once received a dose of castor oil or of sulphate of magnesia. It was, however, but seldom necessary to give a dose of physic, because the food given, and particularly the large quantities of scorched prickly pear which the cattle were compelled to eat, kept the bowels sufficiently loose and prevented constipation. I am convinced the treatment just prescribed contributed much to the mildness of the attacks, and I think it is highly probable, or almost certain, if the cattle on their way from Cairo to Uvalde had received nothing but Northern food and Northern water, or been subjected to the same or a similar treatment as on the ranch, that is, had received the same kind of food and been kept isolated and out of

infected places, they never would have got any Southern fever at all. Although it cannot be doubted that Mr. Abrams's cattle became infected before they reached Uvalde, it must be mentioned that a graded short-horn bull, which either had been raised in Texas or at least had been two years on the range, was a few times let into one of the pens which was occupied by half a dozen heifers; but it must also be stated that at least two of the heifers, one in the same pen and one in another one, showed indications of being affected with the fever before the said bull had ever been in the pen. I am also convinced that the large doses of chinoidine had a very good effect, and very essentially reduced the fever, as is proved by the great reduction of temperature (in one animal from 105.1° F. to 101.5° F. in fourteen hours, and in the others in proportion).

It may be the frequent and rather severe "northers" which prevailed also contributed in mitigating the morbid process, but at least one severe "norther" happened to be blowing when Messrs. Lytle and McDaniels's cattle were sick, and they all died. Moreover, the temperature of those of Mr. Abrams' diseased heifers which did not get any chinoidine was not reduced during the night of January 11-12, in which the "norther" was blowing; neither did the animals to which no chinoidine was given show any general improvement like those which received the medicine. Whether chinoidine is a remedy or not must be determined by further experiments. It certainly is, like quinine and other cinchona alkaloids, a good antiseptic or germicide, and an excellent tonic; and as Texas or Southern cattle fever has a good similarity to certain other zymotic diseases more or less prevalent in the South among human beings, diseases which as a rule yield, if to anything, to large doses of quinine, I have great hopes that chinoidine, if thoroughly tested, will be found to give good results.

EXPERIMENTS.

Although the experiments made in the spring by Professor Burrill and myself with cultivated mesquit grass *bacilli* were not a success, our failure in obtaining satisfactory results cannot be considered as a proof that the track pursued is a wrong one; on the contrary, our want of success—Professor Burrill did not make any inoculations—was undoubtedly due to mistakes committed, to influences left out of consideration, and to adverse circumstances. I therefore concluded to repeat these experiments; but as it was difficult to obtain fresh ox blood when wanted, I chose another medium—Cox's gelatine—which not only was more available, but also promised the advantage of admitting better examination. I prepared an infusion of mesquit grass on August 24. The grass was collected on July 10 on Mr. Rogers' ranch, in Nueces County, and the water used was rain water, caught in a clean porcelain saucer set out in a rain on the tin roof of the porch of a new house after it had been steadily raining for over two hours. But although all pos-

sible precautions were taken to keep the infusion pure, a few monads were afterwards discovered in the infusion when examined under the microscope. When the saucer contained about an ounce and a half of water, about half an ounce of the dry grass was put in and then the whole was again set out in the rain, so as to get an infusion as similar as possible to those prepared by nature in the water pools of the so-called hog-wallow land. The dry grass was soon well soaked, and the saucer when nearly full of water was taken into my room and there kept under cover. The first examination was made at 10 p. m. on August 25, with a Hartnack No. 10 water immersion objective and Beck's No. 2 eye-piece. I found pellicles or zoogloea, masses of micrococci, numerous diplococci and bacteria, some spirilla, and quite a number of large *bacilli* similar in appearance to those obtained before and seen in the morbid tissues, &c., in Texas or Southern cattle fever. I found also a few monads lively swimming through the field. A second examination was made on August 26, at 10 a. m., with Zeiss's $\frac{1}{18}$ homogeneous immersion objective and Beck's eye-piece No. 2. I found numerous *Bacterium termo*, some larger bacteria, some large *bacilli*, and quite a number of monads (see Plate I, drawing No. 1). A third examination was made the same day at 8 p. m., with the same appliances, and revealed some spirilla and the same forms seen before. On August 27 I made a fourth examination, with the same objective and the same eye-piece, and found large and small *bacilli*, large and small bacteria, and quite a number of spirilla (Plate I, drawing No. 2). On August 26 I prepared a solution of Cox's gelatine of convenient consistency, sterilized the same by heating, put some in two clean test-tubes, closed each with a plug of cotton, sterilized again by heating, and then, after the gelatine had become sufficiently cooled, charged each test-tube with a small drop of the mesquit grass infusion. The pipette used had first been heated in the flame of a spirit lamp, and the cotton was moved just enough to enable me to introduce the point of the pipette and let fall a small drop of the infusion. In the morning of August 27 tube No. 1 did not show much change; when examined the solution only contained some zoogloea—masses of micrococci. The solution probably had not sufficiently cooled—was too hot when charged. The contents of tube No. 2, however, were cloudy. A third test-tube, which, like the others, had been filled at the same time to about one-third with sterilized gelatine solution, which, however, had not been charged with anything, appeared to be perfectly transparent, and its contents had not undergone any change whatever. The contents of tube No. 2, when examined, contained a large number of very small bacteria, single and united in twos, but the same, although numerous, kept by themselves, and did not seem to mix with the larger forms, *bacilli*, bacteria, and spirilla. After this I made a new culture every other or every third day, according to the condition (cloudiness) of the contents of my test-tubes, and always used three tubes, numbered 1, 2, and 3. Nos. 1 and

2 were charged with a drop of gelatine, containing cultivated schizophytes, from the tubes bearing the corresponding number, while No. 3 contained nothing but sterilized gelatine, and served as a control tube. On September 2 I commenced to stain the schizophytes with purple aniline on the slide, when examined under the microscope, and found that some of them took the staining very readily, while others could be stained only with difficulty or not at all. As the drawings will show specimens of what was found in each culture, it will not be necessary to give the details of every examination. When I left Fort Worth, on the evening of September 8, I took my charged test-tubes with me and continued the cultivations at Champaign. In the microscopic examinations considerable difficulty arose from the high refractive index of the gelatine solution, and as some of the schizophytes present did not stain at all, or but very poorly, it is possible that some forms were present which remained unobserved. On September 16, after I had finished my other business at Champaign, I inoculated a two-year-old grade Jersey steer, belonging to the Illinois Industrial University, with 30 minims of the sixth culture, by injecting the same with a hypodermic syringe into the subcutaneous connective tissue on the right side of the neck. The temperature of the animal, which had an inflamed scrotum but was otherwise healthy, was 103° F., and taken before the inoculation was made. Professors Morrow and Prentice kindly promised to watch the steer, and to report at once if anything should happen. As till date no report has been received, I suppose the inoculation remained without effect. To be candid, I must say I did not expect the inoculation would cause an infection or be productive of the cattle fever, because a sixth culture cannot be considered as sufficient, but as my duties called me away to Southern Kansas, and as cold weather was fast approaching, I had either to use the sixth culture or abandon the experiment altogether. While in Champaign I also made some other experiments for the purpose of determining by what means Southern cattle infect Northern pastures, but as these too are incomplete, or yet without reliable results, it will not be necessary to give a detailed account of them.

MICROSCOPICAL RESEARCH.

As the results of the microscopical examination in the various cases differed but very little or not at all, it will not be necessary to give the details of each examination. The tissues and secretions examined were the liver, the gall or bile, the kidneys, the spleen, the blood, and the urine.

1. *The liver* (see Plate III).—The morbid changes presented in the liver, microscopically examined, in many respects somewhat resembled those produced by a parenchymatous inflammation. The liver in all cases was found to be very rich in blood or much congested, and considerably swollen. The tissue always contained—was permeated by—a greater or smaller but in all cases a considerable quantity of yellow,

and yet more or less fluid exudation, which usually gave the whole tissue a decidedly yellow tinge. Mixed with this exudation, or also penetrating the hepatic tissue, was always some extravasated blood. In all cases the liver cells were found to have undergone important alterations. The interior of the cells usually presented, particularly if the liver had been artificially hardened in alcohol or bichromate of potash, a granular or somewhat reticulated appearance, and the nuclei, with rare exceptions, were obliterated or could not be seen. The arrangement of the liver cells in the acini or islets was always more or less obscured or disturbed—at least, with rare exceptions, never presented that regularity seen in a healthy liver, because not only the walls of the central veins and of the vascular network but also the *membrana propria* of the liver cells and the walls of the interlobular biliferous ducts were almost invariably permeated with the bile-like exudation, and swelled, or even undergoing dissolution and destruction. The connection between a great many liver cells was therefore often loosened by infiltrated exudation, or even destroyed, which probably explains why some broken-down or disintegrating liver cells could be found in the contents of the gall-bladder. The walls of the larger blood-vessels and biliferous ducts, however, usually appeared to be intact, or if not were but slightly affected, and then only in their interior lining. In some cases the whole liver appeared to be in a state of incipient dissolution; in most cases, however, the tissue, although rather tender and easily broken, was yet tolerably solid. If an incision was made the surface of the cut immediately became moistened with exudation, and the exudation thus obtained for microscopical examination always contained numerous blood corpuscles, a comparatively large number of leucocytes or white blood corpuscles (several in each field), often disintegrating and broken-down liver cells, varying numbers of *bacilli*, and numerous micrococci. The blood corpuscles not yet destroyed or in a state of dissolution presented very often a cremated appearance, and the *bacilli* and micrococci were found to be present just as well immediately after death, though not in such an abundance as one, two, or three days later. The *bacilli*, however, could never be found in perfectly fresh blood taken from a vein or an artery at the *post-mortem* examination of an animal that had just died or been killed by bleeding while in the last stage of the disease. If the exudation was examined when a day or two old it contained a good many irregularly shaped cylindrical bodies of a deep orange color, which stained but very little with aniline, or not enough to lose their characteristic color, and undoubtedly were morbid products, or perhaps some crystals. Sections made from pieces of liver which had been artificially hardened often contained innumerable reddish or deep orange-colored granules and crystals, somewhat resembling those of hæmatoidine, but as a rule less regular in shape, which were not seen in perfectly fresh preparations. On the whole, the extravasated or, perhaps more correctly, exuded blood was always infiltrated into

the tissue, and mixed with or an inseparable part of the exudation. Only here and there a few small circumscribed extravasations could be found deposited in the tissue as small dark-colored specks.

2. *The gall.*—The gall or bile, always very viscous, thick or semi-fluid, of a yellow-greenish brown color, and a grumous or granular looking appearance, contained, microscopically examined, quite a number of *bacilli*, numerous orange or deep orange colored apparently cylindrical rods, either uniform and regular in shape, or irregular and somewhat bent or crooked, and evidently crystals or morbid products, a great many bright yellow or orange colored, granular flakes, a large number of mucus corpuscles and epithelium cells from the hepatic ducts, and some disintegrating and broken-up blood corpuscles and liver cells.

3. *The kidneys.* (See Plate IV.)—The kidneys, although hyperæmic and permeated with exudation, and therefore more or less swollen and loosened in their tissue, particularly in the cortical substance, were found, on the whole, to have pretty well preserved their structure. Under the microscope it was found, however, that many of the glomeruli or Malpighian vascular coils were destroyed, and most or nearly all of the Malpighian canals more or less widened or dilated, and interiorly presenting a somewhat roughened and granular surface, which, it seems, sufficiently explains the presence of the dissolved blood in the urine, and the subsequent red, reddish-brown, or purplish color and the albuminous character of the same. It may be here stated, if the urine was treated in a test-tube with a little nitric acid, and then slightly heated, the coagulated albumen, when settled, always constitutes one-third, and often over one-half, of the contents of the test-tube. Of course, the albumen flakes were loose, and not free from water.

4. *The spleen.* (See Plate V.)—The spleen, which, next to the liver, is undoubtedly one of those organs which in Texas or Southern cattle fever presents the most conspicuous morbid changes, was usually so hyperæmic (filled with blood), enlarged or swollen, and broken up in its tissue as to make it exceedingly difficult, and sometimes impossible, to get from a fresh spleen any section thin enough to show the structural changes under even a low power. In nearly all cases the trabeculi were found to be more or less destroyed, and the pulp to consist mostly of dark-colored or purplish-black blood in a state of dissolution. The blood corpuscles, if not broken up and dissolved—a great many were in every case—nearly always presented a more or less crenated appearance. The blood itself, or, if preferred, the pulp of the spleen, always contained a great many leucocytes or white blood corpuscles, exhibiting a granulated surface, and also contained numerous micrococci and *bacilli*, the latter apparently identical to those in the liver. Pieces of spleen, artificially hardened, and taken from a spleen which was comparatively solid, have not yet been examined, but soon will be, and then the result will be noted, and if of any importance, recorded.

5. *The blood.*—The blood contained in the arteries and veins was

always rather thin and watery, and greatly diminished in quantity, but besides that the fresh blood did not show anything abnormal when examined under the microscope. It may have contained in a given quantity more leucocytes or white blood corpuscles, and being rather thin, less red blood corpuscles than the blood of healthy cattle.

6. *The urine.*—The urine, always exceedingly rich in albumen, and of a red color, the latter varying from a pale red to a dark brown or blackish purple, proved to be in every case perfectly transparent and almost, though not entirely, destitute of red blood corpuscles, because the blood it contained was almost perfectly dissolved. Besides the dissolved blood or its constituents, the urine, when examined under the microscope, contained some epithelium cells, some mucus corpuscles, numerous micrococci, and some *bacilli*, the latter apparently of the same kind as those found in the liver and in the spleen.

As all the other morbid alterations in other parts and organs, for instance in the third and fourth stomach, in the intestines, in the uterus, in the lungs, &c., are not constant, and therefore either more or less inessential or simply necessary consequences of the other constant morbid changes, I did not deem it necessary to subject the same to a microscopic examination, and even if I had desired to do so it would have been next to impossible, unless an examination of the other more essential material, while yet fresh, had been neglected. Diseased tissues of an animal that has died of Texas or Southern cattle fever will rapidly decompose, and cannot be kept many hours unless artificially preserved, and all the *post-mortem* examinations, except the one of Dr. McManigle's cow, near Harper, were made at places (cattle ranches) from 20 to 30 miles distant from my headquarters; consequently the material, in every case except one, had to be carried so far before it could be examined.

CONCLUSIONS ARRIVED AT AND FACTS ASCERTAINED.

As the investigation of so called Texas or Southern cattle fever is not yet completed, the conclusions arrived at and the facts ascertained, which I shall have to offer, do not and cannot cover the whole subject; on the contrary, a good many things remain yet to be proven. Even the real cause of the disease and the means of its workings in the animal are yet a matter of inference; at any rate are not fully known and cannot yet be demonstrated by undeniable facts. Neither can the peculiarities of the infectious principle or the conditions required to produce and to communicate the disease at present be fully explained, notwithstanding the facts are tolerably well known. A great deal, it must be admitted, is yet enveloped in darkness, but the prospect that darkness will be lifted and light appear is a good one. The nature of the infectious principle and the laws which govern its action, however, can only be ascertained by experiments. Fortunately, enough is known to afford a basis for experimentation. Still, notwithstanding the real cause of

the malady cannot yet be demonstrated, important facts have been ascertained and been brought to light. Southern cattle fever, as far as I have been able to learn and to observe, is an *infectious* disease, but is *not contagious*, or, in other words, is never communicated by mere contact; a diseased animal does not give it to a healthy animal, and even such Southern cattle as are able to communicate the disease indirectly or mediately to healthy Northern cattle, do not seem to be able to do so by mere contact, or, at least, if they are, it must be an exceedingly rare occurrence. At any rate, I do not know of any well-authenticated case in which it happened; neither did any one of all those experienced cattle-men in the North and South with whom I conversed or corresponded. The infectious principle, it seems, requires a medium or vehicle outside of the animal organism in which it can develop its malignancy, and which serves as its bearer and is able to cause an infection only if introduced with that outside medium or bearer into the organism of an animal that does not possess immunity; but by causing an infection or producing the cattle fever, it also seems to exhaust its malignancy or its ability to cause further mischief. It may sound rather paradoxical, but nevertheless it is a fact, an animal that conveys the infectious principle and is able to communicate the disease in an indirect way does not take it or does not become diseased itself, while an animal that takes the disease does not communicate it to others, either directly or indirectly. This is not only my experience, but also that of gentlemen who have had for many years abundant opportunities to make observations, and have themselves suffered great losses. I will only mention Mr. Sherlock, of the "Eagle Chief Pool," in the Indian Territory; Mr. McCoy, one of the founders of Abilene, Kans., formerly the great shipping-place of Texas cattle; Mr. Elmore, of Moore & Elmore, Kansas, and also member of a commission firm in Kansas City; Mr. George B. Loving, of the Texas Live Stock Journal, Fort Worth, Texas; Major Hinkle, of the same paper, San Antonio, Texas; Major Drumm, in the Indian Territory, and many others. Further, the infectious principle, whatever its nature may be, is not volatile, cannot fly or be carried from one place to another by winds, because on one side of a fence, even if it is only a barbed wire fence, the pasture or range may be infected, and every animal grazing there may die, while cattle grazing on the other side of the fence do not become infected. The members of the Eagle Chief pool in the Indian Territory, particularly Mr. Ewall and Mr. Sherlock, informed me that their range is separated from the regular Texas cattle trail, over which thousands of Texas cattle are driven every summer to the shipping stations in Southern Kansas, only by a fence of three barbed wires seven miles long, and Texas or Southern cattle fever never affected any of their cattle that remained on their side of the fence unless some Texas cattle broke through, while, on the other hand, every animal that did not respect the fence but broke through

and went on the Texas trail took the disease and died. That such is invariably the case was corroborated by other credible observers.

The period of incubation varies, and is uncertain. Its duration seems to depend more upon the amount or quantity and particularly upon the intensity or malignancy of the infectious principle taken up than upon the individuality of the animals, which, however, is not altogether without influence. Wherever Southern cattle fever makes its appearance in a herd of cattle which have been exposed to infection at the same time and from the same source, the period of incubation, no matter whether long or short, whether two weeks or three months, is, as a rule, of nearly the same duration in the individual animals, and there is seldom more than a few days' difference between the single outbreaks. Usually one or more animals become affected—show plain symptoms—a day or two earlier than the others; then several animals take sick at the same time; and a day or two later, and almost always within a week or ten days after the appearance of the first case, the whole herd, perhaps with the exception of one or a few animals which may remain exempted if the herd is a large one, will plainly show the characteristic symptoms of the fever. A peculiar observation was made by Dr. Mills, of this place, in 1867, when Texas or Southern cattle fever caused so many losses in this (Champaign) County. Dr. Mills found that the period of incubation was usually from four to six weeks or longer if the Northern cattle occupied a range, pasture, &c., on which Texas or other Southern cattle had been grazing, immediately or only a day or two after the Texas or Southern cattle had been taken off, provided the latter had occupied the pasture only a short time, not exceeding a day or two; but that the period of incubation was considerably shortened if some time was allowed to elapse after the Southern cattle had left the range before the Northern cattle were let in, or if the Southern cattle had occupied the premises some length of time—several weeks, for instance. That such actually is the case has been confirmed by my own observations. Northern cattle, if taken South, but particularly if taken to places where the range is thoroughly infected, usually show the first symptoms of the fever in about two or three weeks after their arrival, and in Messrs. Moore and Elmore's herd the period of incubation was also but two weeks or a little over. Mr. Sherlock, who, perhaps, has as much experience in regard to Texas or Southern cattle fever as any man living, has made the same observation. It admits a satisfactory explanation only if the infectious principle propagates or increases in malignancy while in or on its medium outside of the animal organism. The shortest period of incubation known to Mr. Sherlock was fourteen days and three hours, minus the time the animal was actually sick, or, in other words, a time of fourteen days and three hours elapsed between infection and death. If the disease lasted two or three days, as is the rule in malignant cases, the period of incubation would be reduced to eleven or twelve days. The longest period of incubation known to him was three months.

According to Mr. Sherlock, Mr. McCoy, Major Drumm, and others, Southern cattle fever is always the more severe the milder the preceding winter in the South, while after a severe winter cases are as a rule less frequent and the disease itself less malignant. According to all unbiased observers, Southern cattle may with safety be driven North in the winter, or as long as they do not get any new grass on their native range; but if they are driven North after they have had on their native range one good meal of new grass, or, what amounts to precisely the same thing, after the spring rains and warm weather have set in in the South, they will spread death and destruction wherever they go, though they themselves remain healthy, or at least show no symptoms of the fever which they indirectly, by infecting trails, ranges, pastures, water-holes, &c., communicate to other cattle. The immunity possessed by Southern cattle, however, is only a temporary one. Southern or Texas cattle that have been one winter in the North, even if only in Southern Kansas, or the northern part of the Indian Territory, have thereby lost their immunity and are just as susceptible as any other cattle born and raised at the place where the Southerners have passed the winter. Such Southern cattle, if exposed to an infection where they have wintered, or if taken back to the South, will just as readily take the disease and die of it as any Northern cattle. In regard to the cattle fever they may be said to have become Northern cattle (see account of Mr. Campbell's Texas cow). Therefore, as the immunity is only a temporary one, and as a few cases are known—if the disease were not almost invariably fatal there might be more—in which an animal a year or two after it had recovered from the first attack became affected a second time, M. Pasteur's inoculation or vaccination theories need not be tried on Texas or Southern cattle fever.

To what causes or agencies Southern cattle owe their immunity while on their native range or during the first season in the North when coming directly from the South I hope I shall be able to explain more fully than at present in my next report, and therefore will not now attempt it. Where the line has to be drawn between the North and the South—calling South all that country in which the native cattle, if driven or shipped North, are able to communicate the fever to Northern cattle by infecting the pasture-grounds, &c., of the latter, while they themselves remain exempt, and in which cattle imported from parts further north will contract the disease and die of it, and calling North all that country in which the native cattle do not possess immunity, and are not able to indirectly infect other cattle—will be very difficult if not impossible to decide, because the line at best can only be a relative one. Cattle from Southern Texas, for instance, which possess immunity at home, and for one season further north, will, if taken North at the warmer season of the year, commence to spread the disease in localities as far south as Northern Texas, the Panhandle, and the southern part of the Indian Territory, and by infecting the ranges, &c., will give it to

the native cattle of those districts; and these latter cattle, for instance Arkansas, Choctaw, and Cherokee cattle, if shipped or driven North, will begin to spread the fever in localities as far south as the northern part of the Indian Territory and Southern Kansas, and will themselves remain exempt, or become affected only if they should happen to graze or to drink at places that have been frequented or occupied before them by cattle from parts much further south than their native range, for instance by cattle from Southern Texas. Experienced cattle-men and cattle-drovers also told me that they consider it by no means safe to ship cattle from Southern Kansas or the northern part of the Indian Territory in the warm season of the year to parts much further north, for instance to Iowa or Nebraska, because they think there is danger that even these cattle may infect the Northern pastures, &c., and spread the disease. Further, while Northern cattle—cattle from Illinois, for instance, or from other Northern States—if shipped to the South, will almost invariably very soon contract the Southern cattle fever, even if they should not go further south than the southern part of the Indian Territory or Northern Texas; the native cattle of the last-named localities will likewise take it, if the same are shipped or driven to Southern Texas or to places near the Gulf.

TREATMENT AND MEANS OF PREVENTION.

As to the treatment and measures of prevention I have but little to say, for my experiments in that direction have only just been commenced. The treatment of Mr. J. V. Abrams' herd in Uvalde County has been given above, and the following letter, just received (February 9) may tell the rest. Mr. Abrams writes:

UVALDE, TEX., *February 2, 1883.*

Dr. H. J. DETMERS, *Champaign, Ill.:*

SIR: Your favor of 22d ult. at hand. Am pleased to report cattle all doing splendidly. No more sickness after you were here. The cattle that we drenched when they had such a high fever recruited at once, and in a few days showed no signs whatever of having been sick, except in the color of the droppings, which continued to look a little blue or slate-colored for about a week.

With many thanks, &c., I remain, &c.,

J. V. ABRAMS.

The cattle to which he refers as having been drenched received each about six drachms of chinoidine, or about one drachm for every hundred pounds of live-weight. In my letter of the 22d of January I asked Mr. Abrams to report about his cattle, and the above letter is his answer.

As to the prophylactics or means of prevention I am not yet prepared to offer anything reliable that is new, for it is well known that Texas or Southern cattle fever will not make its appearance in the North if no cattle are imported or allowed to come in from the South at those seasons of the year in which it does not sufficiently freeze to kill the

vegetation, and that the disease does not occur in the South unless Northern cattle are imported.

If cattle-breeders in the South, in Texas for instance, desire to improve their native stock by interbreeding with Northern cattle, and are willing to take the risk incurred by importing breeding stock from the North, I advise them to make their importations, if possible, not earlier than the middle of December nor later than the middle of January, because in the South the two last weeks in December and the two first weeks in January are apt to bring more frost or cold weather than any other time of the year, and therefore must be considered as the least dangerous time for importing Northern cattle. Besides offering the above advice, I am as yet unprepared to do more than to give the observations of Mr. Elsner, an experienced and observing stock-raiser in Comal County, Texas, which very nearly coincides with my own views, and are corroborated by the observations of others. Mr. Elsner says it is perfectly safe to import Northern cattle, particularly calves, into Texas, but the animals must not be unloaded on the road, must be fed in the cars with Northern food, and be watered with Northern water taken along from the shipping point, and must be muzzled before they are unloaded, and thus be driven to the point of destination, so as to prevent them from eating anything on the road. At their destination a stable and a yard, the former one that has never been occupied by any Southern (Texas) cattle, and the latter a securely enclosed piece of bare ground, must be ready to receive them on their arrival. When in their stable and yard their food must consist exclusively of green grain or cultivated food plants—the same can be had in Southern Texas, if timely preparations have been made, at all seasons of the year—bran, meal, and scorched prickly pears, and their water for drinking must be drawn from a well or be obtained from a spring. But Mr. Elsner says all precautions will be in vain if the newly arrived cattle are only once allowed, before they have become sufficiently acclimated, to graze on hog-wallow land, or to drink the stagnant water of a hog-wallow or of any other pool or small pond, for in that case they will contract the fever in spite of all precautions. Major Drumm, who has a large cattle ranch in the Indian Territory, 40 miles south of the Kansas line, informed me that Mr. Wilson, in Bee County, Texas, has made the same observations, and holds the same views as Mr. Elsner. That in importing Northern cattle in the South the risk incurred, as a rule, is the greater the older the cattle, and *vice versa*, is a fact pretty well understood by almost every cattleman in Texas. If no extraordinary precautions are taken, and if the cattle imported are full grown, the losses by death often approach, and not seldom amount to, one hundred per cent., while of imported calves, especially if imported in the winter, the losses seldom average more than 25 per cent. Consequently, until other reliable means of prevention can be devised which do not require more labor, care, and expense than Texas cattlemen are willing to take, it will be prudent, if importations

of Northern cattle are to be made, to import none but calves just old enough to be weaned, and to import them only in the coldest season of the year, between the 15th of December and the 15th of January.

In conclusion, I have to protest against the name "*Splenic Fever*," first proposed and given to Southern or Texas cattle fever by Prof. J. Gamgee, for two reasons. The first is, the most important morbid process is invariably going on in the liver and not in the spleen, as will become evident to every one with an unbiased mind and a fair knowledge of general pathology, if he observes and analyzes the symptoms exhibited by the diseased animals, and makes a *post-mortem* examination after they have died, notwithstanding that in some cases the morbid changes in the spleen may be more conspicuous than those in the liver. The second reason is, "*Splenic fever* or *Splenic gangrene*" (*Milzbrand*) is the well-known and well-established name of an entirely different disease. Therefore, even if the morbid affection of the liver in Texas or Southern cattle fever were not of more vital importance to the animal organism than that of the spleen, and primary to, or productive of, the latter, as it appears to be, the name *Splenic fever* would only lead to confusion. The people of Texas also object to the name "Texas fever," or "Texas cattle fever," and look upon it as indicating an unfair partiality toward their State. They say, if Texas cattle infect the pastures, &c., in the North, and thus communicate the disease to Northern cattle, other Southern cattle from other Southern States do precisely the same, and if the cause of the disease has its source in Texas it also has in other parts of the South. The disease, they argue, should therefore not receive its name from a part of the country in which it originates, or is said to originate, but from the whole, and should not be named "*Texas cattle fever*," but "*Southern cattle fever*." Still, although the above argument is unanswerable, Texas is the great cattle-producing State of the South ; more cattle are shipped to the North, West, and East from Texas than from all other Southern States combined, and the disease is so well known by the name of "Texas cattle fever" that it will be impossible to entirely abolish it.

Very respectfully submitted.

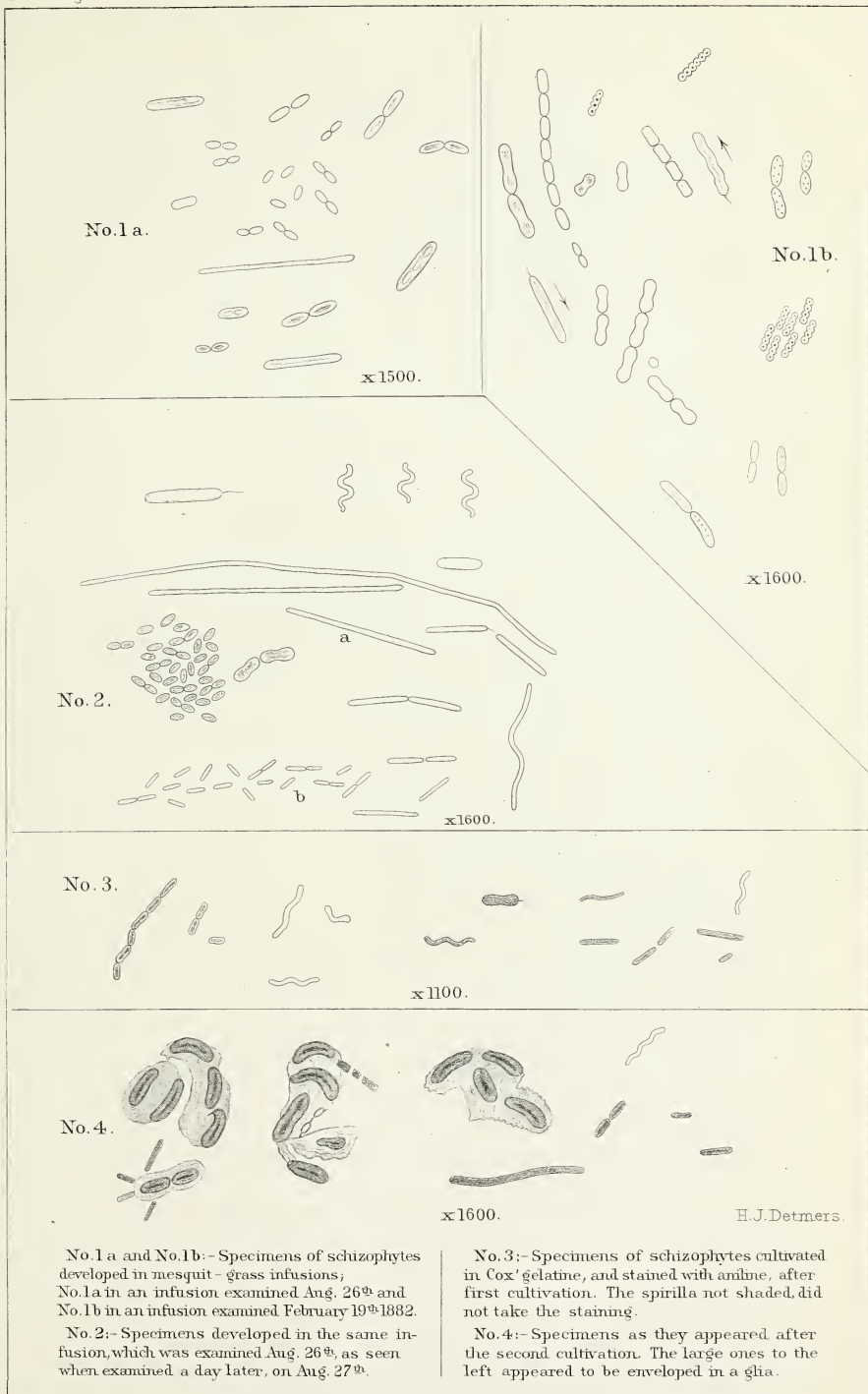
H. J. DETMERS

CHAMPAIGN, ILL., *February 21, 1883.*

TEXAS, OR SOUTHERN CATTLE FEVER.

Investigations of D^r H. J. Detmers.

Plate I.



TEXAS, OR SOUTHERN CATTLE FEVER.

Investigations of D^r H. J. Detmers.

Plate II.

No. 5.



x 1100.

No. 6.



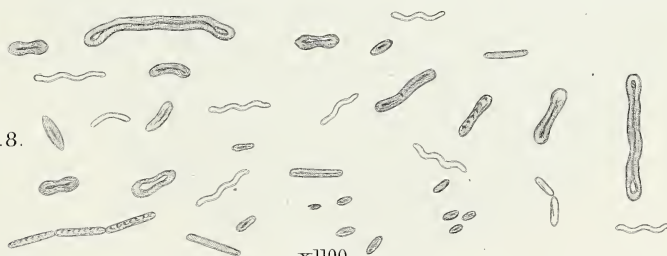
x 1100.

No. 7.



x 1100.

No. 8.



x 1100.

No. 9.



x 1100.

H. J. Detmers.

No. 5:—Stained specimens of the schizophytes from the third culture.

No. 6 and No. 7 b:—Stained specimens of schizophytes from the fifth, and

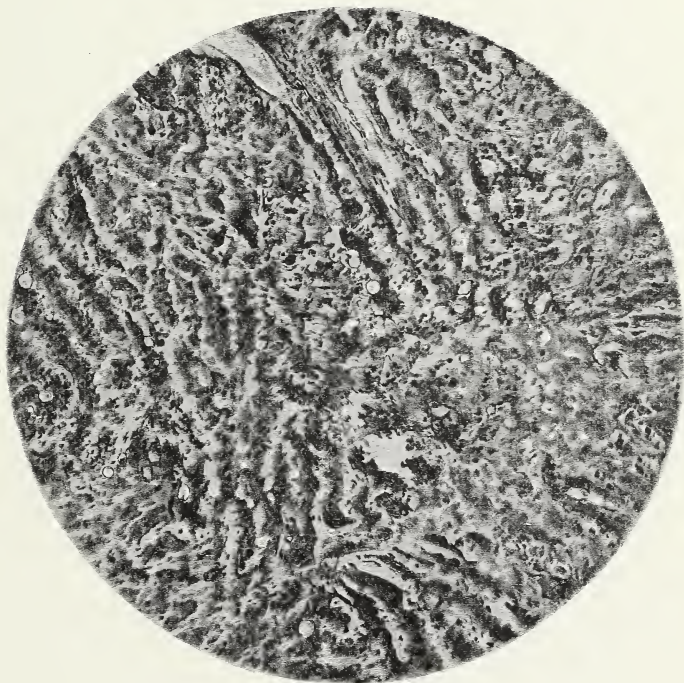
No. 7 a:—From the fourth culture.

No. 8 and No. 9:—Stained specimens from the sixth culture, which was used for inoculation.

TEXAS, OR SOUTHERN CATTLE FEVER.

Investigations of D^r H. J. Detmers.

Plate III.



Microphotograph of Section of Liver Tissue—x 120

TEXAS, OR SOUTHERN CATTLE FEVER.

Investigations of D^r H. J. Detmers.

Plate IV.

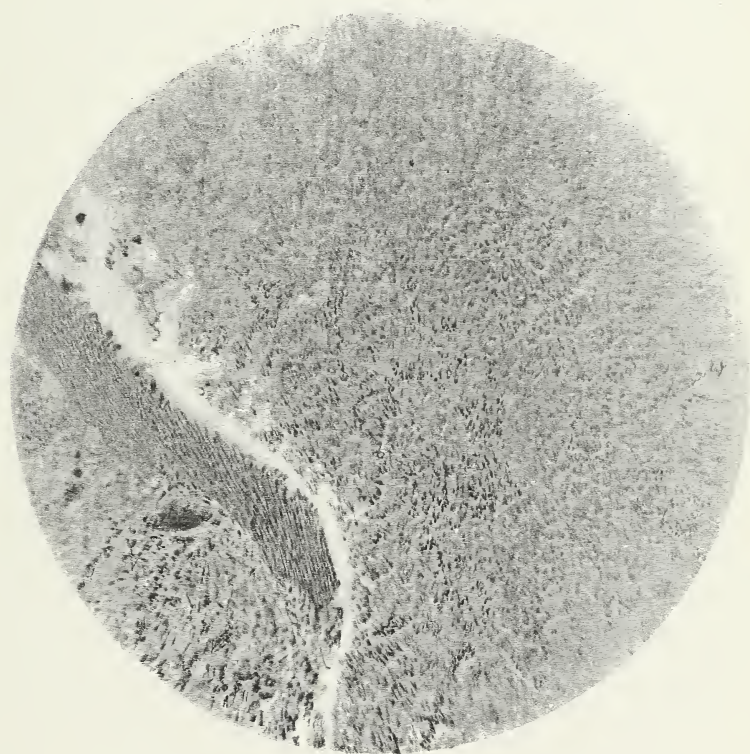


Microphotograph of Section of Kidney—x 120.

TEXAS, OR SOUTHERN CATTLE FEVER.

Investigations of D^r H. J. Detmers.

Plate V.



Microphotograph of Section of Spleen—x 120.

SOUTHERN CATTLE FEVER.

Hon. GEO. B. LORING,
Commissioner of Agriculture :

SIR : I have the honor to transmit the following results of investigations and correspondence relating to the "Cattle Distemper" of Virginia:

During the past summer a disease broke out among the cattle in Southside and Piedmont, Virginia, which, from its severity, fatality, and epidemic character, caused such serious alarm among the farmers and stockmen of that section that they called public meetings, and asked the Commissioner of Agriculture to give them relief by having the dreaded disease investigated by competent persons skilled in veterinary science. The Commissioner responded, and directed Dr. D. E. Salmon to proceed to Virginia and thoroughly investigate the cause of the disease, and, if possible, trace and locate the line of the permanently infected district.

My investigations, as one of the assistants, extended from the Dan River, on the North Carolina line, to the north side of James River, in Henrico County, Virginia. The Virginians claim that this disease originated in North Carolina. So strong were they in that belief in the last century that the colonial legislature, in 1766, passed a very stringent act forbidding the importation of cattle from the colonies of the Carolinas into Virginia.

The act of the legislature is appended to this report in order to show the fear the people of the State had of this cattle disease at that early period. In support of their theory, I give the investigation and experience of Mr. John McDonald, one of the early Scotch settlers on the Cape Fear River, in North Carolina, with the disease in his section just ten years before the Virginia legislature passed the act before mentioned.

With the aid of McDonald's notes we are able to trace the route of the disease from the Scotch settlers on the "Cape Fear" in a northern direction across North Carolina to the Piedmont country bordering on Virginia. From the best information to be obtained, it seems that the disease was brought to the border counties of Carolina by cattle from the Cape Fear country, and from them it spread, as a natural consequence, through the channels of trade and intercourse between the two sections. Starting in Halifax County, which is the first in proximity to North Carolina, its course is traced through that country to Charlotte, which lies northeast, and on through a tier of counties to the northeast running to the James River above and below Richmond, along the public roads traveled through Virginia, leading in the direction of the

Cape Fear country in North Carolina. According to statements made by the early settlers, the disease seems to have been very common. At that period oxen were used principally for transportation purposes, hence it is supposed the belief among the inhabitants of Virginia was founded to some extent on *that fact*—that this disease was brought from North Carolina by oxen in passing from one State to the other. They were allowed to graze on the roadside, and in that way the pasturage along the roads became poisoned.

The people still hold to the belief that the disease had its origin in Carolina, and call it "Carolina Distemper," "Scotch Murrain," and "Carolina Fever." I have endeavored to avail myself of every channel of information possible bearing on the early history of the disease, as well as that relating to the present time, this being necessary to establish the infected line. I have followed its history from the Cape Fear, in Carolina, to the James River, in Virginia, giving the names of the counties and a *line-tracing* through them, showing the route of the infected district. Correspondence from different sections of the State, bearing on the subject, are also furnished, which may prove useful to farmers and stock-raisers.

The first cases of the recent outbreak of the disease were in Halifax and Charlotte Counties. Mr. J. B. McPhail, a prominent citizen and wealthy farmer of Charlotte County, had an outbreak among a herd of native cattle on one of his farms, at the junction of Stanton and Roanoke Rivers, about the 1st of August, which he termed distemper. The original herd numbered 35 head; he lost 9 out of that number. The cause of this outbreak cannot be authentically established, but is supposed to have been brought by oxen coming to the railroad station from an adjacent farm where infected cattle had been pastured. The symptoms as described by Mr. McPhail are as follows: Drooping head, refusing to eat, rapidly losing flesh, drying up of the milk, trembling in the flanks, bowed back, bloody urine, violent constipation of the bowels, temperature of body variable.

Mr. McPhail says:

For many years I have at intervals had what we call bloody murrain or distemper among my cattle. I have discovered, as I believe, a preventive, viz: As soon as the disease appears, give the animals salt, sulphur, and lime at their pleasure. I feel satisfied this remedy prevented the further continuance and spread of the disease on my estates.

Mr. W. P. Dye, a prominent farmer and stock-raiser of Charlotte County, had a herd of 130 native cattle. The disease broke out among them September 6, and continued until the last of October. The symptoms, treatment, and history of the disease has been very intelligently written by Captain Dye, which is presented elsewhere. From this point we trace its course through the following counties: Cumberland, Prince Edward, Nottoway, Amelia, Powhatan, Lunenburg, Goochland, and Chesterfield to James River. Crossing to the north side of

the James, we find it in Henrico County. The disease prevailed alarmingly in Chesterfield and Henrico Counties. It is thought infected cattle were brought to the stock-yards of Manchester and Richmond by dealers and sold to butchers and dairymen, who allowed the animals to roam on the commons and pastures in the vicinity of the cities. On the north side of the river, in Henrico County, the disease broke out among the milch cows about the 20th of June, and has continued until the present time. A few miles below Richmond are located a number of dairy farms. The milch cows of the different dairymen are herded together in the morning after milking and turned over to a herder whose business it is to keep them together and graze them on the best pasturage to be found. I noticed one man who was watching one hundred and fifty cows belonging to twenty different persons. He grazed them on the commons and public pastures where the butcher grazed his beef cattle. These pastures were evidently poisoned by cattle brought from infected districts by dealers. Mrs. Idle, a dairywoman, had 9 milch cows, and they contracted the disease in June and September; she lost 5 out of the 9. I was informed by an intelligent farmer that within a radius of twenty miles around Richmond dairymen, farmers, and cattle dealers had lost over two hundred and sixty head of cattle during the past summer and fall—from the date of the outbreak in June to the 20th of November—by this disease. I have a number of memorandums of persons who have lost from one to three, and in many cases all they had. It is the popular belief by persons who are familiar with all the circumstances connected with the malady in this section that the commons and pastures were poisoned by cattle being brought here and pastured from infected districts, in different parts of the State and North Carolina. *Post mortems* of several of the animals which died revealed the following: The kidneys and spleen congested; bladder filled with bloody urine; gall bladder distended far beyond normal size, and covered with a brown-colored fatty substance; liver much swollen, and of a yellowish cast; stomach natural; intestines constipated, and filled with hard feces. In most cases the kidneys were seriously diseased.

The following is the act passed by the colonial legislature of Virginia in November, 1766, in the seventh year of King George III, for the preservation of the breed of cattle:

Whereas it is represented to this general assembly that the inhabitants of this colony have sustained great damage in the loss of their stock of cattle, reason of distance passed, cattle being brought into and carried through this colony from the provinces of North and South Carolina:

Be it enacted by the lieutenant-governor, council, and burgess of this present general assembly, and is hereby enacted by the authority of the same: That from and after the last day of April next (1767), and during the continuance of this act, every person intending to drive any cattle within this colony except such as shall be brought from Great Britain, shall immediately upon his arrival within this colony apply to a justice of the peace and deliver to him an account of the number of cattle in his drove, and such justice shall and may, and is hereby required, to administer to the person so

applying, the following oath, to-wit: "I, A. B., do solemnly swear that I have known all the cattle now by me brought into the colony of Virginia for the space of twenty days last past, and none of them, to my knowledge or belief, are now or have been affected with any contagious distemper within that time, so help me God."

And thereupon such justice shall immediately issue his warrant directed to any two freeholders within his county, who shall view the same and make due report thereat to the said justice as soon as may be; and if such viewers shall report that the same are to their belief free from every contagious distemper, such justice shall thereupon immediately give the driver of such cattle a certificate or bill of health for the same, expressing therein the number of cattle, together with a description of the drivers attending them, and if any justice shall grant a certificate or bill of health contrary to, the justice shall forfeit and pay the sum of ten pounds, to be recovered by an action of debt in any court of record within this colony.

From and after the said last day of April next, 1767, during the continuance of this act, it shall not be lawful for any person to bring any cattle into this colony from any place whatsoever, except Great Britain, without having obtained a certificate or bill of health. Every driver passing through this colony shall be obliged to produce a certificate or bill of health to any freeholder demanding to see the same; such driver refusing to produce the same when demanded, it shall be presumed that said cattle are illegally brought into the colony. No inhabitant of this colony shall permit any infectious beast to go at large, but shall confine the same apart from the rest of his cattle till the same shall be perfectly recovered. If any such distempered beast shall die, the owner shall burn and bury the carcass. Any owner permitting such distempered cattle to go at large after he shall have known the same to be infected, shall pay the sum of six pounds for every such beast so permitted to run at large. This act shall not extend to the counties of Southampton, Isle of Wight, Nansmond, or Norfolk.

AN ACT for the preventing infection of horned cattle into the Commonwealth of Virginia, passed October, 1785, 10th year of the Commonwealth.

And be it enacted by general assembly, That the driving cattle into or through the Commonwealth or any part thereof, if it be not to remove them from one plantation to another of the same owner, or to be used at his house, shall be deemed a nuisance, unless the driver shall produce to any freeholder of a county wherein the drove is passing, who shall require it, a bill of health, signed by some justice of the Commonwealth, containing the number of the drove, with description of the cattle, by their sexes, flesh marks and ear marks, or brands, and certifying them to be free from distemper; or notwithstanding he may produce such bill of health, unless he shall forthwith obtain another of the like requisition, if any such freeholder make affidavit before a justice that he hath cause to suspect some of the cattle to be distempered, such bill of health shall not be given, in either case, before two disinterested freeholders, appointed by warrant of a justice, shall have viewed the cattle and reported them to be free from distemper.

A freeholder refusing to obey such warrant shall be amerced by the justice granting such warrant, in any sum not exceeding twenty-five shillings. If the cattle appear by the report to be distempered, the owner may impound them, and if he refuse to do so, or if he suffer them to escape from the pound, before a justice shall have certified that they may be removed without annoying others, the same justice, or some to whom information shall be given of the fact, shall by his order cause them to be slaughtered, and their carcasses, with the hides on, but so cut or mangled that none may be tempted to take them up and flay them, to be buried four feet deep. Those that shall be employed in executing such orders shall receive five shillings for every head so buried, to be paid by the county wherein it shall happen, and every one appointed by the order, who shall refuse or neglect to execute it, shall be amerced in the sum of five shillings for every head so ordered to be buried. Every one shall so restrain his distempered cattle, or such as are under his care, as that they may not

go at large off the land to which they belong; and when they die shall bury them with their hides in manner aforesaid; and knowingly offending in either of these instances shall be amerced in the sum of twenty shillings for every head they shall neglect so to bury.

This act shall commence and be in force from and after the 1st of January, 1787.

AN ACT for the preventing infection of horned cattle into the Commonwealth of Virginia, passed December 28, 1792.

And be it further enacted, That the arriving of cattle into or through the Commonwealth or any part thereof, if it be not to remove them from one plantation to another of the same owner, or to be used at his house, shall be deemed a nuisance, unless the driver shall produce to any freeholder of a county wherein the drove is passing, who shall require it, a bill of health signed by some justice of the Commonwealth, containing the number of the drove, with descriptions of the cattle, by their sexes, flesh marks and ear marks, or brands, and certifying them to be free from distemper; or notwithstanding he may produce such bill of health, unless he shall forthwith obtain another of the like requisition, if any freeholder shall make affidavit before a justice that he hath cause to suspect some of the cattle to be distempered, such bill of health shall not be given, in either case, before two disinterested freeholders, appointed by warrant of a justice, shall have viewed the cattle and reported them to be free from distemper. A freeholder refusing to obey such warrant shall be amerced by the justice granting such warrant, in any sum not exceeding five dollars. If the cattle appear by the report to be distempered, the owner may impound them, and if he refuse to do so, or if he suffer them to escape from the pound, before a justice shall have certified that they may be removed without annoying others, &c.

Mr. John McDonald, in his sketches of the early Scotch colonists of Cape Fear, N. C., gives the following observations on the cattle disease in that section, which proved so fatal to their foreign stock of fine cattle. Mr. McDonald was a great fancier of stock in Scotland before coming to America. He says:

The Scotch colonists brought from Europe some blooded cattle for breeding purposes, with a view of improving the stock. Soon after the arrival of the cattle on the soil of their new home (on the Cape Fear), it was noticed that two of the finest young cows of the foreign stock were sick; when first discovered they were standing alone from the herd in remote portions of the cow-pasture with their heads bowed towards the earth, ears drooped forward, with back bowed as if drawn up from severe pain. The hind feet were drawn well under the fore feet. Such was their condition when first found by the person who attended to the cattle.

Mr. McDonald made an examination of the animals, and found the bodies hot; horns of a changeable temperature, first hot and then cold; breathing deep, heavy, and labored, which seemed to exhaust the animal's strength. They were very restless, but could not walk without great difficulty, and then only at a slow, reeling gait. Thirst for cold water was very great. Sometimes before drinking water it was common to hear them bellow as if in great agony. Water seemed to give temporary relief. The urine was frequent, copious, and of a dark bloody character. He opened the carcasses of these cows after death, and found the following anatomical condition: Bladder filled with a dark-colored fluid almost the color of blood; liver enlarged; spleen and kidneys much congested; gall-bladder filled with a dark thick fluid. The cavity of the throat contained dark venous blood.

Heart natural, but entirely empty of blood; left lung and pleura seriously diseased; right lung far advanced into hepatization. He fails to give treatment. The disease runs from one to three days. He says the mortality was so great among the cattle that the colonists moved their stock to the Piedmont country, which is near the Virginia line. The cattle taken from the Cape Fear section to the Piedmont regions became healthy and thrifty, while the native cattle of Piedmont became diseased and died—that is, the cattle of the up-country that mingled with those from the lower tide-water (Scotch cattle).

McDonald believed the disease climatic, and maintained that it was peculiarly adapted to the tide-water and middle sections of the Atlantic Coast States. He became fully satisfied after years of experience that the disease was contracted from herds in lower climates when brought in contact with those from a higher latitude.

Mr. Albert Dunn, a farmer who lived in Chesterfield County, near Petersburg, Va., gave special attention to raising cattle of the Durham breed. In a communication written by him he says:

It is a remarkable fact that numerous deaths of cattle were caused in his neighborhood by the disease commonly called the "distemper," and in Prince George County, not 20 miles south of here, it is said that very few cases have been known for many years. Cattle brought from the North or West are more exposed to the fatal disease, from which there is rarely a recovery.

Dr. W. S. Morton, a prominent physician of Cumberland County, Virginia, in a treatise on distemper among the cattle of that State, June, 1854, says (omitting a detail of facts which led to the opinion):

I have been fully convinced that the disease is infectious, and only so by the saliva of one animal getting into the mouth of another. Unless this occurs, I do not believe that any contiguity of a sound animal to one affected with this disease, or even to one having died of it, will cause him to contract it. I believe it is generally admitted that the same law governs the communication of distemper to one horse from another, and that he cannot take it unless he bite the same grass, eat out of the same manger, wear the same bridle, or in some way get tainted in his mouth.

2. I have fully satisfied myself that cattle, or at least some of them, which have once had distemper, even for years after they have appeared to be well, can communicate it to such as never had it. This law of disease also prevails in the one called glanders in horses. As among glandered horses, so some cattle wear marks of disease during life, such as a dripping of yellow water from the nose, inaptitude to shed off their hair in summer, although fat, and occasional failure to give milk for a day or too, whilst slight disease is manifest. Perfect severance of such horses from all sound ones or, more safely, killing them, stops the spread of the disease. The same expedients, especially killing tainted cattle at the proper season of the year, would be followed by the same result.

3. Commons or other lands on which distempered cattle have roamed, until their bones have almost whitened the ground, if well inclosed during fall or winter, may be considered as safe from distemper ever afterwards, unless causes of re-infection be allowed. This I consider as fully established by striking examples coming under my own observation. It is, then, very important, especially when so many high-priced cattle are coming into the country, that these rules should be practically remembered.

I would on no account permit a sound animal to eat grass, or even dry food, (at the season of the year when distemper prevails) in company with one which was

ever had been infected. Indeed, I should be quite chary in buying hay, should I unfortunately ever need that article, from an infected farm or district. One case of infection has come to my knowledge for which I could find no other cause.

4. I have not known distemper to occur between the middle of December and the 1st of June. It is then possible that new comers into the herd might be protected by separation from the infected, even just before the last-mentioned period.

For many years I kept two herds on the same farm, one infected, the other sound. A man with a *white skin* was seen to pull and leave down the barrier between them. Unfortunately before matters could be rectified, a sound ox walked grazing for nearly fifty yards on the infected grass. He died of distemper in a few days. I kept those two herds separate from the last of June until nearly Christmas. In December, 1836, I removed from the neighborhood of Farmville to this county, selling all the cattle which had been exposed, preferring to buy such as might be proof against the disease in future. I brought the protected favorites to Cumberland, and have had no distemper here, but if I begin to relate facts I shall violate my promise of brevity.

5. As regards the cure, I put no confidence in the nostrums so loudly applauded by ignorance and credulity. I have observed that many cattle which discharge only dark-greenish or black urine, resembling strong copperas water, are apt to recover whether physiced or not, but when the urine was bloody I never knew one to get well. I have generally endeavored to purge them, when costive, with a pound of "Glauber's-salt," or nearly a pint of spirits of turpentine, to release them from the annoyance of gray-headed biting-flies which are sure to be attracted to them by myriads. I have, when I could, put them in a dark house; when this was impracticable I have covered them over with the twigs and leaves of elder, spicewood, sassafras, or any others whose scent will disguise and conceal their smell from the flies, or have had them well rubbed with bruised horse-mint or pennyroyal. I have also applied ice to the hollow behind their horns, and rubbed it along the course of the spine when there was great heat of skin, and in a few instances soon after the attack, with apparently the happiest effect. If not already in the shade, an arbor should be raised over them. Many ways of guarding them have been devised, besides that surest preventive, a careful separation from infected cattle. The basis of most of these is common salt, unlimited access to which is doubtless beneficial to all cattle. I had once a friend living in a neighborhood peculiarly afflicted with distemper. He placed under a shelter in his pasture, for the use of his cattle at their pleasure, a large mass of clay, strongly impregnated with equal parts of salt and air-slaked lime. Although he boasted of thus guarding his cattle from distemper, I found he most sedulously prevented all intercourse with their infected neighbors.

The law requiring the carcasses of cattle dying from distemper to be deeply buried or buried unskinned—the skins however are generally worthless—being founded in ignorance of the laws of propagation in that disease, is worse than useless, and with every *dead-letter* law ought to be repealed. It leads many without faith in its value to obey it scrupulously, because it is a law of the land. I have not a doubt but that with a knowledge of the laws of propagation in distemper, as developed in the foregoing rules, every proprietor could exterminate it from his premises were a wisely constructed fence law only enacted, but such a matter as this is beneath the notice of politicians.

It is a curious fact, in the history of this disease, that although it has existed so long in our State, it lingers only about our towns and within a few miles of the roads leading to cattle markets in this and more Northern States, along which cattle from Georgia and the Carolinas were formerly driven. I believe it soon dies out in neighborhoods where the cattle are closely confined in pastures; although it has been thus smothered, and in many places exterminated, I fear it will break out with redoubled fury when blooded cattle shall be transmitted through the State, and bulls of improved breeds begin to go from house to house. I would warn my brethren, remote from the above mentioned roads and from localities affected by the disease, to watch

closely, lest the enormity of such an evil come upon them. I would exhort all sending animals to the cattle shows to guard them most carefully against grazing by the use of well-made muzzles. These if properly attended to will save them, unless they chance to get a mouthful of hay or other food which has been slavered on by some infected animal.

I know very little of what is called "bloody murrain" in Scotland and other parts of Great Britain, but I strongly suspect, from the mystery and destruction by which it has so much aroused the alarms of superstition, that it was carried to North Carolina by Scottish immigrants, being brought *here* afterwards; it received, like many European men, a new name, "North Carolina distemper." If this be so, I would gladly restore the old name, as I feel disposed to associate something better than the distemper with the name of the good old North State.

The heavy injuries sometimes produced by this disease render it a much more formidable affair than those unacquainted with it would suppose. I would therefore implore intelligent practical farmers, and especially the officers of our State Agricultural Society, scrutinizingly to search into and, if possible, suppress the evil. Whole neighborhoods sometimes lose all their cattle and, a little milk, much less cream, cannot be procured to put into coffee. I have known great mischief caused by the transmission of blooded cattle through the country at a wrong season of the year. Efforts to improve our stock are very laudable, but again I warn those who make them to use muzzles as safeguards, or, which is much better, to do it during those months when the disease seems to be uncommunicable.

Mr. William R. Hatchett, of Caswell County, North Carolina, writing on the cattle diseases of that section, July 24, 1854, says that his father, who lived in Charlotte County, Virginia, had the disease among his herd for three years continuously; he continuously prevented all intercourse with the infected cattle of the neighborhood, as the disease was considered contagious. After this period, in consequence of an imperfect fence, his cattle would frequently get with distempered stock, and roam on lands which diseased cattle had grazed on and died, yet his cattle escaped this contagious and fatal disease which raged in that section six or eight years, but finally died out.

Precautionary directions should be scrupulously observed by all who are introducing fine cattle in the State, especially in that portion where this disease has once existed. The disease has been prevailing in this surrounding neighborhood for the past eight years.

When I moved from Charlotte County, Virginia, I brought my oxen and continued my former treatment of clay and salt, with the exception of one year (1851), when I lost two of my oxen. I brought from Virginia three milch cows, a bull, and heifer with the disease; the present stock of my milch cows were free from disease, as it had not reached the neighborhood from whence I purchased them. I have, with the exception of the years of 1852 and 1853, carefully prevented my cattle from getting with those of my neighbors; with all my precaution, united with the neglect of my former treatment, my cattle by some means contracted this disease, and the result was as above stated. I am much biased in favor of Dr. Morton's suggestion that what we term the North Carolina distemper is probably the bloody murrain of Scotland, and was brought to this State by Scottish immigrants.

A gentleman in this section, now dead, who formerly traded in Fayetteville, in this State, informed me that the first case of distemper he ever heard of in this State was near Fayetteville, which was settled by the Scotch. We may safely infer that this disease originated from infected cattle brought over by the Scotch immigration.

A correspondent writing from near Beattisford, Lincoln County, North Carolina, October, 1851, to the *Southern Cultivator*, says :

Distemper among cattle in this section seems to be caused by undue exposure to the hot sun; a bare pasture with little shade is therefore unfavorable.

It is found that when an animal is first attacked it separates from the herd, seeks some retired spot, hangs its head, and ceases to ruminate. If the animal be discovered within four or five hours after these symptoms appear the disease may be arrested by drenching with a decoction of poke-root. The urine at this stage is reddish; a few hours later it is bloody; when this occurs no cure is known; the animal expires in twenty-four hours.

When cattle are furnished with an unlimited supply of clean, strong ashes and salt they usually escape. The disease usually breaks out in this section in June.

Mr. James Davis, Evergreen Hill, Polk County, Texas, on September 6, 1851, writing on the cattle disease of the South, says:

I would recommend as a treatment for murrain in cattle to drench the animal with a strong decoction of peach-tree leaves, produced by boiling; use from one quart to one gallon. Before discovering the above remedy, I resorted to the treatment of the veterinary physician, which is by blood-letting and purgatives, and changing the cattle to poor pasturage.

What is called the murrain in this portion of the South with which I am acquainted differs widely from what is called the murrain in Europe. It is there considered inflammatory to some extent, but not so much so as in Europe; consequently it requires different treatment.

A gentleman in Madison County, Virginia, gives the following on the cattle disease in that section of the State:

I will favor you with a full description of the symptoms of the disease which has proved so fatal to the cattle in a large district of this county. I will here remark that I have had some experience and made some observations with my own stock and those of my neighbors since the year 1845, for I have escaped its ravages one year only since that time up to the present. The cause that produced the disease is so obscured that I believe no one, at least in my knowledge, has as yet been able to detect it; it remains a mystery hidden in the arcanæ of nature, to be discovered only by patient research and philosophical induction. It is possible that chemistry, assisted by physiology, may yet reveal the mystery. Almost every farmer who has observed this disease has some favorite theory, and, unfortunately for the advancement of truth, scarcely any two of us will agree. Among the popular causes assigned for this disease, I will mention a few: The extensive broom-sedge fields uninclosed that in a great measure supply our cattle with pasturage during the spring and summer months. Others attribute it to the immense number of ticks that adhere to our cattle; while others say it is for want of salt. These with a thousand other causes are assigned which are too frivolous to mention. I am satisfied that the causes assigned above, together with all others I have heard ascribed, are unfounded, destitute of any foundation in truth. They are mere opinions hastily formed, and are often, as they deserve to be, hastily discarded. It has been among my cattle since 1845, as I before remarked, every year except 1848, and during that year I pastured my cattle on broom-sedge alone, and they were never healthier; they were fat all the season, and as fine beef as our market affords. Now if the pasturing on broom-sedge will produce murrain, why were my cattle free from it when they grazed on broom-sedge and that alone? During that season one of my neighbors, in sight, lost thirty-two head; they grazed as my stock did, and drank the same kind of water (limestone). To attribute it to the ticks is equally unfounded, if not ridiculous, for I have had my cattle to die of it that never had a tick on them. It cannot be regarded as contagious, for I have known the cattle of

one man to pass directly among those of another which had it, and graze around the dead carcasses of those which have died of it, and yet escape its ravages.

Symptoms.—The eyes are weak and languid; the horns are cold generally and hollow; excessively prostrated, and in attempting to walk they stagger as though they were under the influence of buckeye or ivy. The whole nervous system appears to be prostrated and the faculties of the secretory organs suspended. A cow may milk well in the morning, and if attacked during the day no milk will be found secreted in the udder at night. In a few hours after the attack the ears hang as though the animal had neither the energy nor ability to elevate them. They are not disposed to eat or drink. On a *post-mortem* examination the brain is disorganized, altered in appearance and color; the kidneys in a high state of inflammation; the bladder filled with urine of a bloody hue; the stomach frequently disorganized, and filled with a red or yellow fluid; in fact, the whole animal organization presents an appearance differing widely from those that die with any other disease. This disease appears to be peculiarly fatal to milch cows. I have never known a cow attacked while giving milk that ever recovered. I need not mention any of the remedies that I have used, as none of them have ever been successful. I have attempted to sketch briefly the development of this disease. If I have been successful in any degree, so that any of your correspondents can offer any antidote that may result in arresting it, I shall feel that I have accomplished that which I greatly desire.

Mr. Yardly Taylor, a farmer of Loudoun County, Virginia, in reviewing the essay of Dr. Morton, of Cumberland County, that State, on the cattle disease of the South, gives the following in support of Dr. Morton's theory:

In perusing the essay of Dr. W. S. Morton on the disease of cattle I was reminded of circumstances of this disease which occurred here a few years ago that seemed strongly to confirm the conclusions arrived at in that essay. Some years ago a connection of mine from Ohio took a drove of horses to Baltimore and sold them. He then purchased a drove of mules and conveyed them to the southern part of this State, in Southampton or the adjacent counties, and there disposed of them, and knowing that the graziers of this county were not fully supplied with cattle he bought a drove of the small cattle of that region and brought them into Loudoun. It being before pasture was plenty, he brought them to my barn-yard and fed them with hay until they were sold. During the summer almost every individual who bought cattle out of this drove had the disease introduced among their stock; not one of the Southern cattle died of it. It seems that infected cattle grazing on grass will communicate the disease to healthy cattle that graze on the same grass.

I bought a heifer that had a calf while here; my milch cows passed daily through the barn-yard where the cattle were, but did not feed with them, and none of my cattle were attacked with the disease. Some of my neighbors lost about half a dozen cattle of more value than any they purchased out of this low-country drove. The Southern cattle fattened kindly, but did not increase much in size. Were another drove to come from that region I question very much whether our farmers, from their present experience, would suffer them to enter inclosures, much less purchase.

The disease has not appeared since that season.

The following is a communication from General William C. Wickham, of Virginia:

SIR: I have your circular, and propose to answer your questions as follows:

First. I never learned that the movements of cattle from my section (the head of tide-water and north of James River) to any point north or west of us was followed by the outbreak of any disease along their track or at their final destination. On the contrary, I have known cattle moved with entire success a hundred miles westward,

and I have no apprehension of bringing disease amongst my cattle by buying cattle at any point east of me.

Second. My experience is entirely adverse to bringing cattle over six months old from points north or west of me. I have hardly known an instance of cattle being brought from a distance of even twenty miles west to my immediate section, Hanover Court-House, that they have not died the first summer from what we know in that section as "distemper," "murrain," or "bloody urine"—the three terms being synonymous.

Third. Until recently I had never heard the disease attributed at all to ticks, and have therefore made no examination. I some years ago lost heavily of my finest cattle on good clover fields, where I do not think there were any ticks. It is a fact, however, that the disease is more general amongst cattle which graze upon what we call the commons, consisting of woodlands, roadsides, swamps, and piney old fields, in which ticks abound.

Fourth. I have known this disease for fifty years past, and never but once knew it appear as early as the month of June. It prevails to a greater or less extent amongst the cattle in my section, on the commons, every year, and generally from the middle of July to hard frost. On the inclosed land, where the cattle are not allowed to roam outside of the inclosures, the disease now very rarely appears, which I attribute very much to the non-intermixture with other cattle and to preventives which I use. These were formerly sulphur and salt mixed, say one part sulphur and six parts salt, spread upon tar poured into shallow troughs. More lately I have used a mixture recommended by Mr. Frank G. Ruffin, of which the formula is as follows: 1 gallon salt, say 42 gallons; $\frac{1}{2}$ pint sulphur, say 7 quarts, 11 pounds; $\frac{1}{2}$ pint saltpeter, say 10 quarts, 16 pounds; $\frac{1}{2}$ gill copperas, say $2\frac{1}{2}$ quarts, $4\frac{1}{2}$ pounds. All to be well mixed.

In the case that I refer to of the loss of cattle by distemper in the month of June I attributed the loss to the fact that I had not commenced soon enough in the season with the sulphur and tar, which I was then using. My rule now is to commence with this preventive as soon as I take my cattle out of their winter quarters. I have been entirely successful in bringing calves from Wythe County, Greenbrier County, and Augusta County at weaning time, turning them with my other calves, and then letting them share the same fortunes with those born on the farm, and have found even when brought in the spring (though I prefer bringing them in the fall) that they become acclimated and stand the summers as well as the native-born calves. I have tried it with yearlings, giving them a pasture to themselves, so that they might not be mixed with any other stock, and have lost them.

I think the disease is more common and probably fatal in dry and hot weather than in other periods of the summer. I have tried various remedies after the cattle have been taken with the disease, but have not satisfied myself that the mortality is any less with those that are treated than with those that are left to take their chances. It has been a rule at my house for the past sixty years, inaugurated by my father and continued by myself, never to allow oxen to go off the farm, our opinion being that the biting of the grass on the roadside, where roaming cattle had grazed, was likely to be productive of the disease. As a general rule when the disease appears it sweeps off pretty much the whole herd, but I have frequently known instances in which only one or two animals in a herd of fifty or sixty were affected.

I endeavor to have diseased animals immediately separated from the herd. The foregoing is the result of my practical experience and observation in the matter. I do not know anything whatever of the scientific points of this subject.

Very respectfully, your obedient servant,

WM. C. WICKHAM.

Mr. Glover, of Buckingham County (which lies north of Halifax), who lives eight miles from the court-house, had a herd of 45 cattle. He had purchased 30 head, and of that number there were two yearlings bought

in Halifax County, which borders on the North Carolina line. These two cattle were bought in June and turned among his herd. On the 8th of July the disease, or North Carolina fever, as it is called by the people of that section, made its appearance, the symptoms being the same as before described. In two days the disease proved fatal. He lost nine of his native cattle by the malady. From the best evidence to be obtained, these two animals were originally brought from one of the infected counties in North Carolina to Halifax County, Virginia, and from there brought to this county.

Mr. Pryor, of the same county, Buckingham, who lives 4 miles distant from the court-house, had a herd of five native cattle. When the disease made its appearance among his cattle in September, about two months later than it appeared among Mr. Glover's, he lost four of the five native cattle. The cause of the outbreak was poisoned pasture by a drove of cattle brought from Halifax County, Virginia, and counties in North Carolina within the infected district.

Mr. Julian N. Ruffin, of Hanover County, Virginia, who resides 15 miles northeast of Richmond, has handled cattle for several years, and has had no disease among his stock. He has dealt principally in animals bought in the adjoining counties. In New Kent County, 8 miles below him, the disease made its appearance last fall and swept away an entire herd. The cattle of Hanover, with a few exceptions, escaped the recent epidemic so far as we have been able to learn. In New Kent, the adjoining county, it prevailed to some extent. Caroline County, which joins Hanover on the northeast, has been free from it. Col. John Washington, who lives in that county, 46 miles north of Richmond, is an extensive stock-raiser. He has a herd of pure-blood graded Jerseys and Short-Horns. He says he never had any disease among his cattle, although he has purchased a large number at different times.

Mr. A. P. Rowe, of Spottsylvania County, who lives 60 miles south of Washington, D. C., is a large stock raiser and dealer, and handles several hundred head of cattle during the year. He is well acquainted with the farmers and stockmen of this county; says he has had no disease among any of his animals, and does not know of any cases having occurred in the county. Says he went to Richmond last September to buy some Jersey cattle from Mr. John B. Davis, who has a stock farm some miles southeast of Richmond. On his arrival at the farm he found two young Jersey heifers (for which he had previously offered \$300) sick with bloody murrain, and within a few days they both died. He intended to purchase the entire herd, but after finding these two sick he declined, fearing that the disease might be carried among his herd in Spottsylvania County. This herd was purchased a few weeks later by Mr. S. C. Kent, of West Grove, Chester County, Pennsylvania, who lives 40 miles from Philadelphia and 60 from Baltimore. Mr. Kent says that up to this date (December 26, 1882) the herd of Virginia or

Davis' Jerseys are doing well, and that no trace of any disease has been visible among his stock.

From the best information to be obtained, the disease did not prevail last summer in the counties of Hanover, Louisa, Spottsylvania, and Stafford.

In most of the cases where it occurred in the counties north of Halifax, Charlotte, and Pittsylvania Counties the cattle had either been driven from those counties or had been bought in North Carolina and pastured in Virginia.

Those familiar with the history of the disease in those counties hold to the idea that it was originally brought from the Cape Fear country, in North Carolina, one hundred and forty years ago.

The following is a communication addressed to the writer from Col. Frank G. Ruffin, of Virginia, who was many years editor of the leading agricultural journal of that State and a prominent farmer and stock-raiser:

SIR: Having been requested by you, as a representative of the United States Agricultural Department, to contribute some information in regard to the diseases that affect our cattle in Virginia, and especially those forms of maladies which may resemble what is known in some other sections of the United States as the Texas or Spanish fever, I beg leave to submit the following paper:

For several years, beginning with 1851, I had the honor of conducting the Southern Planter, a periodical which was the accepted organ of the farmers of Virginia. In that time my attention was frequently called to the diseases of our cattle in those sections of the State where there was or had been any serious disease. On looking into it I soon found that not much was known upon the subject.

The reason was plain. Over the infected districts our rural economy did not take much account of cattle. In the tide-water country, where "red water" was the characteristic disease of a sickly season, the staples we grew were so easily produced and so cheaply transported to market by water that there was comparatively but little money in cattle in any shape. A short, quick rotation with clover was thought more profitable than a longer course with pasture, and Indian corn was worth too much in cash to be fed to beesves. So at least they reasoned, and with much force.

In the Southside country, above tide, nearly up to the mountains, where a somewhat different disease was found, was the largest tobacco-growing region of the State, and there was still less inducement to stock husbandry as a source of profit. In certain of those counties, as for instance Charlotte and Halifax, the cultivation of tobacco on a large scale gave, in addition to other crops, a total value in excess of all the agricultural products of Huntingdon, the model county of New Jersey, and of a similarly reported county of Pennsylvania, a fact which proves how profitable then agriculture must have been. I knew this, for I made the comparison several years ago with the aid of an accomplished statistician. In the whole of Piedmont, from Maryland to North Carolina, there was never any epidemic cattle disease, neither was there in the valley, nor in the southwest, nor, as I have heard, in the Northern Neck, the whole of which is tide-water.

There was not then much knowledge of the disease, because its occasional fatality, though an inconvenience, was never a great disaster; its range was always narrow; there being no veterinarian, the invalid cattle were generally turned over to be treated by the cow-doctor, with the usual consequences of empiricism. Still, the matter attracted the occasional attention of several able men, especially of some physicians, and two of those of more than ordinary skill and reputation contributed their observations and opinions on it to the public one of them, the late Dr. W. S. Morton,

through the late Mr. Edward Ruffin, the agricultural commissioner of the State of Virginia by appointment of the executive committee of the Virginia State Agricultural Society; the other the late Dr. John P. Mettauer, in a memoir on the summer distemper of cows and oxen, addressed to the Virginia State Agricultural Society. The first of these will be found in the Southern Planter for 1855, vol. xv, p. 118. The second in the same periodical, vol. xvi, p. 307. To them I refer you for the best accounts of the disease as it appeared in Southside Virginia above tide-water. My own experience of cattle distemper was in a part of tide-water, and my observation was limited very much to my own farm that I purchased in 1853, and found it in very bad order, whether as respected its agricultural or sanitary condition; and in two years six out of twelve head of cattle that I had introduced from the upper country were dead of this disease. In the absence of veterinary or other reliable aid, I consulted Youatt's work on cattle (the English edition of 1838), in which I found, under the respective heads of *murrain*, *red water*, and *black water*, amid much that would perplex any non-professional student, enough to satisfy me that the disease that the cattle in my district had was of a distinctly malarious as distinguished from a typhoid character. Such as it was it sometimes attacked the cattle that were native to the farm, but almost invariably those that had been brought on it from only a few miles above, even though from a locality under equal supposed malarious influence.

The same thing took place with cattle brought from a longer distance, as from the vicinity of Baltimore, for instance. But there was in such cases no greater certainty or violence of attack than those obtained nearer home, and these facts I think are general to the whole infected tide-water region. After groping about in the dark I at last found or rather stumbled upon a preventive and a remedy, and I think any one in possession of them may, with common prudence, keep his cattle free from this disease or easily cure them if occasionally stricken.

The preventive is a mixture of the following ingredients and in the following proportions: Salt, $\frac{1}{2}$ gallon; saltpeter, $\frac{1}{2}$ pint; sulphur, $\frac{1}{2}$ pint; copperas, $\frac{1}{2}$ gill; to be kept in lieu of other salt, always available to the cattle. Mine was in the open air, in a trough made of two planks mitred together, and resting in brackets on the ground, from the 1st of May to the 1st of November, and as much longer as you please. The use of that mixture (to which, by the bye, my sheep and horses had unlimited access) kept my cattle perfectly free from malarious disease and enabled me to bring upon my farm all that it suited me to bring there. I recollect now, and cite as instances of this (1), an in-calf heifer that came to me in June from the mountains of Orange County and lived to old age; (2) six four-year-old fat cattle that came to me in August from the top of the Blue Ridge Mountains, in Albemarle County, 100 miles away, and kept in perfect health until I sold them to the butcher in the following January; (3) six cattle that I bought from a county above me and kept for two years in perfect health, when I sold them, three of the lot to cow-keepers in the town of Manchester, who turned them all out to graze upon the bare common with upwards of 150 other cows, some of them dying occasionally, and where two out of the three died of red water. Sometimes when from carelessness the use of this mixture was neglected a case or two of disease would appear, but that would always cease when I returned to the preventive.

Sometime after I had been using this preventive I learned, I forget where, of a remedy which is extremely simple, but which I found effective in every case to which it was applied. It consists in giving two or three large boluses of sugar (or a drench of a half gallon of molasses in place of it), to be repeated at intervals of an hour or two until a sufficient action upon the bowels has been produced; and then in giving a teaspoonful of calomel, to be followed if necessary by some mild purgative; this treatment to be pursued as soon as possible after the animal is taken sick. In all the cases that I knew to be treated in this manner the animal recovered, and sometimes without the use of the calomel.

I never had but one cow attacked on my own farm after I had learned of this remedy. That was in 1864, when I had found such difficulty in getting sulphur and

salt-peter that I ceased to use them. That cow was drenched by one of my slaves with sorghum. She got well without further treatment, and on returning to my preventive the rest of my herd escaped. Whilst all this experience was being acquired, my farm had been drained, limed, clovered, and otherwise improved, and may be thought to have acquired comparative immunity from this cause, but a small portion of it was inclosed to herd Confederate beef cattle on in 1864, when many of them died.

This is all that I can tell you from my own observation of any cattle disease among us that can be thought at all climatic.

From those alarming epidemics that exist elsewhere, we are, as far as I know, exempt in Virginia. Our great cattle districts are, I think, remarkably healthy, and tide-water and south-side Virginia, both eminently adapted to cattle-raising, should not be discouraged in the effort to introduce this branch of husbandry from fear of disorders as easily prevented and cured as those which are described by the gentlemen to whose essays I have referred you, or which are more briefly spoken of by myself.

In conclusion I offer you an article copied verbatim from the Southern Planter, vol. xii, p. 5, on hollow horn in cattle. It was contributed by the late Dr. Charles Minor, of Charlottesville, Albemarle County, who was a physician of great ability. I call your attention to it as possessing real value, and as being in strong contrast to the nonsense that has been published on this subject.

FRANK G. RUFFIN.

A writer in the Farm Register, of Buckingham County, Virginia, November 5, 1883, under the above date, says:

I was formerly a resident of the lower portion of Virginia, where murrain was fatal to cattle. I tried many remedies (to which I was advised) without success. Discovering that the bladders of the cattle that died were invariably filled with bloody urine, I determined to try what effect bleeding would have, and found that it was by far the best remedy I had used. When bleeding was resorted to as soon as the cattle appeared sick the remedy was generally successful.

A distinguished writer on distemper among cattle in Virginia says:

I believe the opinion has heretofore most generally prevailed that the disease has spread from the effluvia produced by the carcasses of animals dying of it, and from their bones. This belief, I suppose, gave origin to the law requiring that such carcasses should be effectually buried or burned, and forbidding the preservation or tanning of their skins. I have seen my dogs bring the bones of cattle dead from distemper among my own, which had not been exposed. I was at first alarmed for the consequences, but no trouble ensued. Having an extensive common near me, I have permitted the greater part of my cattle to range at large, and have retained in an inclosure a few favorites, together with my working oxen and those intended for such; these two portions of stock have been very cautiously kept asunder. The disease for several years destroyed some of the first, while the latter were entirely exempt from it, until one of the oxen broke the fence and grazed for a short time among the exposed cattle. He died in a few days, of distemper. It may be said that all admit cattle may take the disease from grazing with the infected, but that this does not imply that they cannot take it in any other way. Nature, though rich in means, is economical in using them, and we generally find but one cause used in the production of any effect. This cause in the propagation of infectious diseases is usually something generated in the sick and applied either by actual contact or in the form of effluvia to some secreting surface of the well. From what was said above it appears probable that this distemper of cattle is hardly produced by effluvia, and I infer that a sufficient cause may be sought in the application of one cow to the mouth of another from her eating food previously besmeared with it.

In confirmation of this opinion I will state a few facts whose bearing on this sub-

ject you will readily infer. I have been credibly informed of a wealthy gentleman who, more than twenty years ago, on finding that distemper had invaded his herd of cattle, ordered that every animal seized with it should be confined in a particular lot, never to come out alive. Those that died of distemper were deeply buried, and such as recovered were fattened and slaughtered for beef. This prompt measure is said to have succeeded by a perfect exemption from disease.

Two other gentlemen within my own knowledge have inclosed extensive parcels of land almost literally whitened with the bones of cattle which died of distemper and made pastures of the very lands. This included one of these pastures which has enjoyed perfect exemption from the disease for ten or twelve years. The other had his cattle infected by getting on a common after total exemption for about the same term.

In districts where the disease prevails it is generally known that there is no danger to oxen in however exposed situations they may be driven, provided they are kept well muzzled. This precaution was much observed in this section some years ago, but the terrors of the disease wore away with its novelty, and we have become more careless. I believe that the cow which has once suffered from distemper is liable to slight annual returns of it, and it may be possible that she is only capable of communicating it during these returns.

I have known two cows that appeared to have distemper slightly every summer. One of them, though apparently healthy, had a yellow liquid continually dripping from her nose, and her hair had a dead appearance even when she was fat. Suspicious of keeping up the disease among the stock fell so heavily on her that I caused her to be killed about four years ago, since which time I have known but one case of distemper among my cattle, and that occurred a few days ago to a cow running on the exposed common.

On the treatment of the disease I have but little to say. The symptoms on which I have chiefly placed my hopes of recovery is the character of the urine. Whenever this assumes the appearance of a solution of copperas, although sometimes nearly black, I expect recovery; when the urine is bloody I forebode speedy death. I have known no cures effected. The same symptoms occur among the cattle of Scotland affected with the disease they call bloody murrain. It is said that the distemper among our cattle was brought by drovers from North Carolina. Some parts of that State were settled almost entirely by a Scottish population. The writer thinks the disease was brought from Scotland by the earlier Scotch settlers of the Cape Fear region of North Carolina by their cattle from that country.

The disease seems to take deeper hold than merely on the blood. There is derangement in all the secretory organs—the stomach, liver, kidneys, and indeed every important organ is frequently gangrenous, and in one instance I saw blood oozing through the skin like drops of sweat before death. The means for prevention promises but little more than those for cure.

These distempers may be called a summer disease, occurring, according to my observations, between the 1st of June and the 1st of December. It is very certain that hot weather operates powerfully in exciting the virus to action. From my own observations, I would judge the period between exposure and attack to be within the limits of three or four weeks, and that an animal having escaped for this length of time is in no danger until a fresh exposure.

If it should be found, on further investigation, that the disease owes its continued existence to those animals solely which have been subjects of it, then there would be hope of exterminating it by removing from our farms every one on which suspicion could possibly rest, and supplying their places by an entirely new stock.

BUREAU OF IMMIGRATION OF VIRGINIA, GENERAL AGENT'S OFFICE,
Richmond, Va., October 20, 1882.

SIR: For twenty-five years I lived on a farm in Nottoway County, in the southern part of Virginia, on which I kept a herd of about 50 cattle during hot, dry summers.

Occasionally a few of the cattle would be afflicted with a distemper resembling in all its symptoms malarial fever, but it never became *epidemic*.

The affected animal would generally be found in some secluded spot, from which it would not move unless driven. In most instances it would die here if not speedily attended to. The symptoms would be great sulkiness, indisposition to move, great shrinkage of the flesh, no appetite, high fever, producing blisters on the upper lip, highly colored urine. Death would ensue in from three to seven days unless remedies were applied in the early stages.

After carefully examining the cases, I came to the conclusion that the causes of the disease in the animal were the same as those producing bilious fever in man. I therefore treated the disease by administering like remedies to those used by the most skilled physicians for man, which, when administered promptly, so far as I now recollect, never failed to effect a cure.

This treatment was 60 grains calomel in a bolus or pill, mixed in bread or dough, given to the animals. In the course of ten hours a quart of water in which a teacupful of Glauber or Epsom salts and common salt, half and half, had been dissolved. Then 50 grains quinine, keeping up the latter in reduced doses for several days till recovery. It is rarely ever the case that cattle are moved, especially from a higher to a lower locality in East Virginia, in which this disease does not affect them. I never had a Devon of any age, from six weeks to six years old, brought from northern latitudes, which did not take it. I have known Jerseys that escaped. The following was adopted many years ago as a preventive which has, without exception, acted successfully in my own case, and wheresoever it has been tried, so far as I now recollect, viz., half bushel of air-slacked lime, one pound of powdered sulphur, half bushel of common salt incorporated with an equal quantity of woods' earth. This mess can be kept in a close trough where the cattle can reach it at will. By placing salt on it they will soon be induced to lick it. After which cease salting them, but take care to renew the mess as fast as it is consumed. This should be kept in their pens from the 1st of June to November. It is a commonly received opinion that cattle kept stabled during the heated term never have this disease. As to this I cannot speak from experience.

Very respectfully,

RICH'D IRBY.

•Mr. J. S. GOULD read a paper before a session of the "American Social Science Association," which met in Albany, N. Y., in February, 1869, on Texas cattle diseases, from which we make the following extract:

The Texas cattle disease, though new in the State of New York and in the New England States when it made its appearance in 1868, was by no means a new thing in the West, it having been known there since 1854, when Texas cattle were first brought into Illinois.

At that early day the Texas cattle themselves were to all appearances quite well, but the native cattle that grazed on the same pasture, or that drank at the same streams, or that traveled over the same roads, contracted the disease and great numbers died. In 1860 the outbreak of the rebellion effectually stopped the Texas cattle trade, and with it the disease also ceased. In 1868 the trade was again actively reopened and the disease made its appearance. In the early part of August in the year 1868 a very peculiar diarrhoea and dysentery appeared, mostly among laboring men and mechanics and their families.

In New York the usual remedies seemed to have no effect, and many died; while the physicians were thus baffled, it came to the knowledge of the board of health that a large number of cattle were sick at Communipaw with a severe but unknown disease. Dr. Harris, the register of the board of health, who had ample opportunities to study the yellow fever, and who, as health officer of the port of New York, had seen and

studied every form of malignant diseases, went over to Communipaw with a full staff of assistants and made a very thorough examination of the sick cattle.

He saw at once that it was a disease unknown to veterinary science, having a very striking analogy with yellow fever as manifested in the human subject. In view of these facts, which were reported by Dr. Harris, the New York board of health ordered that all meat coming into the city should be inspected before being offered for sale.

The board authorized Dr. Harris to take all the necessary measures for ascertaining the nature of the disease and the best methods of securing exemption from its ravages. Dr. Harris, with admirable sagacity and foresight, immediately organized a system for ascertaining the history and pathology of the disease and its relations to human disorders.

He assigned three eminent gentlemen in their profession to this work, in the following order: one in charge of the microscopical department, one in the pathological, and the other in the chemical.

These investigations were satisfactory, and showed clearly that the study of epizootics is very important. They came to the conclusion that in Texas cattle diseases—Spanish fever, distemper, or bloody murrain—is a disease of the blood. They also establish the fact that these Texas bullocks carry the disease to the native cattle along their route. During the past season, says Mr. Gould (in 1867), out of 4,000 native cattle in and around Tolono, Ill., all but 250 died of the disease (Texas fever) communicated by Texas cattle.

Dr. Jones, of Tippecanoe County, Indiana, bought a drove of 100 Texas bullocks brought by the way of Cairo. He put them with his native herd about the 1st of July, 1868. In three weeks his own herd began to sicken and died of the Texas fever, and he lost 124 out of 160 bullocks.

This is an example of the several losses suffered by Western graziers from the Texas cattle disease, all of which might be avoided if the cattle were transported with humanity from Texas to Cairo, the point to which they are shipped. It was for a long time supposed that none but Texas cattle could communicate the disease, but it is now fully demonstrated that native cattle can and do communicate it to each other.

Respectfully submitted.

J. M. HINES.

JANUARY, 1883.

SOUTHERN OR TEXAS FEVER OF CATTLE.

The following letters and telegrams calling the attention of the Department to the prevalence of Southern or Texas fever of cattle were received during the past summer:

STATE BOARD OF HEALTH OF WEST VIRGINIA,

OFFICE OF SECRETARY,

Wheeling, W. Va., August 7, 1882.

Hon. GEO. B. LORING,

Commissioner of Agriculture:

SIR: An exceedingly fatal, specific, communicable fever of cattle has appeared in the county of Brooke, West Virginia, and the State board of health has been appealed to for assistance. The disease made its appearance about the 20th of last month among native cattle soon after the herding among them of several lots shipped directly from Alabama. The symptoms point unmistakably to splenic fever. If you have any published reports on the subject later than the "Special Report No. 22, 1880," please be kind enough to forward me copies, and much oblige

Your obedient servant,

JAMES E. REEVES, M. D.,

Secretary S. B. H., W. Va.

TRENTON, N. J., August 14, 1882.

General E. A. CARMAN,

Acting Commissioner Agriculture:

SIR: I have the honor to report that on Friday and Saturday last I visited several herds of cattle in North Heidelberg Township, and the two adjoining townships in Berks County, Pennsylvania, and made full inquiry as to the cattle disease in those localities. The number of deaths has been about 30. I found but two cattle sick—one on a farm never having had the disease, and the other where there had been three deaths. Both of these cases were light, and as I saw them were not thoroughly typical of the disease as described. I tried to buy one of the cows for slaughter, but thought \$50, the price asked, too much to pay. Dr. Bridge, who is employed by the cattle commissioner of Pennsylvania, had seen one of the cases the day before. I will ascertain his views. I think a *post-mortem* examination of one of the animals, if others die, will be necessary. No one who has yet seen can prove the cases to be anthrax or splenic fever, and yet there are some facts that would justify suspicion. One of the animals I saw had a decided cough. The only veterinary graduate in that section of the country had not seen the cases. The disease is probably sporadic, and yet needs careful watching and special examination if other cases occur. I think the cattle were all clover fed with the second crop. Two farmers drew my attention to a minute worm in the clover heads, which they said could be found by swarms, but had never been seen before last summer. I have taken the address of the farmers (who all speak German and but little English), and if you think best will be telegraphed if any deaths and have accurate autopsy made. I will write to Dr. Bridge, who is one of the Pennsylvania inspectors, if you desire to investigate still more fully.

Very respectfully,

E. M. HUNT, M. D.

PITTSBURGH, August 15, 1882.

Hon. GEO. B. LORING,

Commissioner of Agriculture:

SIR: I have had 16 cattle die on my farm at Steubenville, Jefferson County, Ohio, in a few days. Plenty sick; what will I do with them? Bought them in Saint Louis. Fear it will spread. I refer you to J. T. Updegraff, member of Congress, as to who I am.

Yours, truly,

JAS. MAIRS.

STEUBENVILLE, August 21, 1882.

Hon. GEO. B. LORING,

Commissioner of Agriculture:

SIR: Dr. Miller, of Camden, N. J., has been at my farm investigating the disease prevailing among my cattle, and no doubt has reported to your Department. Is there any means by which the State or United States can do anything for me, as I think they will all die and possibly infect all cattle in our county. Let me hear from you. Since the doctor left they seem worse.

Yours, truly,

JAMES MAIRS.

AUBURN, N. Y., August 30, 1882.

Hon. GEO. B. LORING,

Commissioner of Agriculture:

SIR: The Texas cattle fever has made its appearance in this section, and many of our finest cattle are dying. Will you please send me the annual report of the Commissioner of Agriculture for the year 1880, also Contagious diseases of domestic animals, 1880-'81.

Yours, respectfully,

JOHN CHOATE, Sheriff,
By E. P. STIELES, Deputy.

BRIDGEWATER, MCCOOK COUNTY, DAK., *September 4, 1882.*

HON. GEO. B. LORING,

Commissioner of Agriculture:

SIR: About one week ago a disease broke out in a herd of 150 cattle, which is proving very fatal. Four have already died and five are now sick. The symptoms of the malady are as follows: The animal refuses to eat and drink, has a dull, dispirited look, a high fever, and has no disposition to move about. The secretion of milk is suspended from the first. The breathing is frequent, sometimes panting. No other symptoms are more marked than the trembling and twitching of the muscles and the unsteady gait of the animal. The hind quarters seem to be most affected, indicated by a difficulty in getting up. The animals die in from two to four days. I was called upon when the third one was taken ill with the disease, and learned that the symptoms of those which died were the same as of the one I have described. Soon after death I made an examination of the third one, and also the fourth, and found the conditions the same, to wit: The liver considerably enlarged, and of a darker color than usual; the spleen of a dark mottled color, presenting a peculiar appearance; the gall bladder very large, and filled with a dark brown, curdled, bloody substance; the kidneys dark and congested, with considerable inflammation in surrounding parts, and also along the spine; the fourth stomach considerably congested; the bladder was filled with dark brown urine; the lungs appeared healthy, with no morbid changes in any of the organs of the chest.

Yours, truly,

E. W. TERRILL.

CHAMPAIGN, ILL., *September 16, 1882.*

HON. GEO. B. LORING,

Commissioner of Agriculture:

SIR: On the morning of the 8th instant, while in Fort Worth, Tex., I bought a copy of the Saint Louis Globe-Democrat dated the 6th, and saw a dispatch from Champaign, Ill., dated September 5, which read about as follows: "Texas fever has been introduced in Champaign, and is prevailing to an alarming extent. A large number of cattle have died, and many more are expected to die." As Texas cattle fever made its appearance in Champaign two years ago, and prevailed in the neighborhood of that city in 1867 to a really alarming extent, I credited the statement, and at once telegraphed to my friend, the Hon. Jas. R. Scott, at Champaign, president of the Illinois State board of agriculture, saying: "See in Globe-Democrat that Texas fever is prevailing in Champaign. If an error, answer at once. If not, shall leave to-night for Champaign." Not receiving any answer, I took the 10.10 p. m. train for Champaign, where I arrived early Monday morning following. I soon learned that several town cows had died under suspicious circumstances, while some were reported to be sick, but when I hunted them up I found they had either died or recovered. I also heard many rumors of some Cherokee cattle having been brought to town and driven over the commons, but was at first unable to learn particulars. Only a few days ago I learned from Mr. Chester, a prominent and very intelligent farmer residing in this neighborhood, that in the latter part of June a drove of either Texas or Cherokee cattle had been unloaded at this place and grazed, though probably only for a short time, on the public highways in the outskirts of this city, particularly on Springfield avenue, or, as formerly called, Springfield road, a street or road considerably frequented by some of the town cows. All those cows which recently died or were sick are reported to have been covered with (Texas) ticks, and some of them (those that died) are said to have passed bloody water or red-colored urine. I therefore concluded to make as thorough an investigation as possible. This I have done, and have come to the conclusion that two, or possibly three, cows—one the property of Mrs. Keegan, one block north of Springfield road; the second belonging to Mr.

Eickelberg, nearly opposite to Mrs. Keegan; and the third owned by Mrs. Price, in a block adjoining the Springfield road—very likely died of Texas cattle fever, while in regard to all others I not only have my doubts, but also have not been able to find any proof that the same have been affected with that disease. They seemed to have died from entirely different causes—eating too much green corn, &c. Even as to the three cows especially mentioned, I consider it probable, and I may say highly probable, but not positive, that they died of Texas fever, because I have to base my diagnosis upon those men who saw the animals while alive, and made the *post-mortem* examinations after they had died. The *post-mortem* examinations of the two cows first mentioned were made by men who do not claim to possess any veterinary knowledge, but whose veracity cannot be doubted, while the *post-mortem* examination of the last-mentioned animal was made by a person who pretends to be a veterinary surgeon. If the disease which affected the other cows (those that recovered) was also Texas cattle fever, it must have been of an uncommonly mild type—milder than I ever saw it. As no deaths occurred after my arrival (Monday morning, September 11), I had no opportunity to make any *post-mortem* examination, and therefore, as already said, have to rely on information received from others.

Once here, I concluded to make use of the opportunity, and inoculated a two-year-old steer (Jersey breed) placed at my disposition by Professor Morrow, of the Illinois Industrial University, with cultivated schizophytes, suspected of constituting the cause of the disease and cultivated while I was in Texas. Further, as carloads of Texas cattle are passing through here at least twice a week, and as Professor Morrow is willing to let me have another steer or two, I intend to make another experiment with material (saliva) from the apparently healthy Texas cattle passing through here. If no damage is done to the steers there will be no charges, but if they take the disease and die I agreed to pay their actual value. As soon as the second experiment (inoculation) is made, I shall leave the animals in charge of my friends, Professor Morrow, Dr. Prentice, and Professor Burrill, and at once go back to the Southwest.

Very respectfully,

H. J. DETMERS.

BRIDGEWATER, MCCOOK COUNTY, DAK., September 17, 1882.

General E. A. CARMAN,

Acting Commissioner of Agriculture:

SIR: I have just received yours of the 9th, asking if any Texas or Arkansas cattle have been imported into our county during the past season. In answer I will say that none have been brought from the States named, but I find that one M. B. Murphy shipped from Lee County, Mississippi, some 31 head of cattle, known here as Cherokee cattle. They were introduced into the herd (spoken of in my communication of August 31), on July 16, and since that time they have been perfectly healthy. On August 23 one cow was taken sick, and on the 25th died. Others were taken sick in rapid succession, until at the present time 26 have died, and 10 more are sick. Two have recovered. I advised the owners to separate the afflicted cattle from the rest, and remove them from the old feeding grounds, and give the following once a day: I dram carbolic acid, in a bran mash. One week ago to-day there were 7 deaths, and 12 sick. During the week there were 8 deaths, and to-day we have 6 or 7 sick; so you see the disease is somewhat on the decline.

I am confident of the nature of the disease and its origin, but some are in doubt as to my conclusions, and I wish you to name the disease, and tell us how it is transmitted from one herd to another. The man who owns the 31 head of cattle claims that as they are not sick, they could not have given the disease to the remainder.

Very respectfully,

E. W. TERRILL,
Statistical Correspondent.

CHARLTON, N. Y., *October 6, 1882.*

HON. GEO. B. LORING,

Commissioner of Agriculture:

SIR: Texas or splenic fever of cattle broke out in the month of August, on the farm of John Buys. As soon as I learned that there was sickness among his cattle, I visited Mr. Buys, and became satisfied from the appearance of the animals that the disease was Texas or splenic fever. I informed the governor of the State at once of my impressions, and he promptly ordered the State cattle commissioner with the State veterinarian to inspect the herd. They did so and reported the disease to be what I had supposed. One animal belonging to John M. Veeder, which was nearly dead, was killed and a *post-mortem* examination made. The spleen was found to be very much enlarged, and decomposition so advanced that it would hardly hold together. The manyplus or third stomach was found to be packed and very much inflamed. The State authorities, under the laws of the State, ordered all persons on whose farms the disease had appeared to keep the cattle confined in the fields and out of the highways, and not to allow them to drink from any stream which passed through other farms. They were also directed to bury all animals which should die, and to maintain these precautions until after a heavy frost. Animals which are sick refuse to eat—go away by themselves, and if possible find some wet and cool place where they lie down and refuse to move. They run at the eyes and the nostrils, and indicate a highly inflammatory condition of the whole system. The disease runs its course, if fatal, in a few days. It is attended with severe pain, which is indicated by the appearance of the animal and its groans. Emaciation follows rapidly, and in the last stages the extremities are cold. Mr. John M. Veeder had one cow very sick, which he separated from his herd, putting her in an orchard. A high wind immediately afterwards blew off a great many apples, which tempted the cow's appetite, and she ate greedily of them. Instead of injuring her they produced a looseness of the bowels and the cow recovered, although Mr. Veeder had supposed that she would certainly die.

This disease has continued up to the present time, abating in cool weather and breaking out anew when the weather was hot. It has been confined to the farms of John Buys and John M. Veeder. I believe the disease to have been introduced on the farm of John Buys by a dozen steers which came over the New York Central Railroad from Michigan. They were on the farm between two and three months before any of the native cattle were affected. None of the steers have shown any symptoms of the disease. They are of Texas or Cherokee blood. There is a rumor that some steers in the same carload died. The disease was communicated to the cattle of John M. Veeder by them getting into the field adjoining where the steers and Mr. Buys's cattle were running. Several cattle belonging to Alexander Sherman and N. J. Crawford have also died, they having pastured with the cattle of Mr. Buys. Mr. Veeder's cattle were only in the pasture of Mr. Buys twice, and then for a very short time. He has lost as a consequence three head, and had several others very sick. It is a remarkable fact that while the cattle of Mr. Veeder were sick they were driven along the same lane and also the highway and ran in an adjoining field with his brother's cattle and none of the latter have been affected. No animals have been sick except those exposed in the same pasture to the Texas cattle, and it seems that native cattle in whatever stage of the disease do not transmit it. Ten cattle in all have died on these two farms.

Respectfully, yours,

F. D. CURTIS,
*New York State Statistical Agent.*MOSSINGFORD, CHARLOTTE COUNTY, VIRGINIA, *October 7, 1882.*

HON. GEO. B. LORING,

Commissioner of Agriculture:

SIR: Yours of the 2d instant has just reached me, having been misdirected, and I hasten to comply with your request, hoping the Department may find the cause of

and cure for the fearful scourge which threatens destruction to one of the most promising interests of agriculture in this section.

On the 9th day of September a graded yearling Shorthorn bull was found dead in the pasture (containing two more yearlings and twenty calves), too much decomposed for a *post mortem*. Buried him without opening. Same night a five-months-old calf, in same lot, found sick. Symptoms: Dull and stupid; separated from the herd; lying with muzzle on the ground; ears pendulous; when aroused seemed stiff; walking with forward legs wide apart; swinging forward part from side to side; head drooping; disinclination for food or drink; trembling; nose and other visible membranes pale, cool, and clammy; extremities cold; pulse small and frequent; respiration hurried. Not knowing disease, placed animal in hospital and let die in the interest of science. Dead forty-eight hours from development of first symptoms.

Autopsy.—Lungs slightly inflamed, not adherent; no pus or serum in chest; gall bladder distended to double the normal size; substance much thickened and covered with fatty secretions; the whole a nut-brownish color; contents overflowing, discolored adjacent membranes; liver swollen, entire substance yellow or gingerbread color; kidneys inflamed bladder full, contents normal; manyplus full, somewhat engorged, contents nowhere indurated, and but little drier than normal; stomach full and normal, small intestines somewhat constipated, feces dark color.

September 12.—Two calves found sick, with symptoms above described. Treated as follows: Powdered chlorate of potass., 3 ounces; powdered hyposulphite of soda, 4 ounces; powdered sulphate of iron, 2 ounces; powdered ginger, 7 ounces—mixed. Dose: 1 ounce night and morning in 1 pint of warm flaxseed tea. To cool and relieve the small intestines, about 24 ounces of strong castile-soap suds was thrown into the rectum after each drench. In each case accompanying the enema was voided hard balls of feces, dark in color, with mucus adherent. After third treatment the bowels became very much relaxed, discharges being copious, mixed with mucus, and very offensive. The third day both returned to their feed and are apparently well. I now thought the disease local, attributing it to a fall grass, growing plentifully in the pasture, having an irregular hard seed, which were found in large quantities in the manyplus. I therefore changed the calves to a new seeded field, but found on the 16th another yearling bull sick. Symptoms same; treatment same. Died on morning of the 17th. Autopsy revealed serum in cavity of the chest; right lung badly inflamed; left lung adherent to sternum; smell offensive; other organs as in case No. 1, except liver not saturated with contents of gall bladder. No. 3 found sick. At 7 o'clock a. m. took food; separated from the herd; seemed inclined to lie down. By advice of a neighbor, at 12 o'clock, took one quart of blood from the neck vein; drenched with half pint lard, and gave half pint lard in injection. Died at 7.30 p. m. *Post mortem* showed lungs much diseased; pleura discolored; spleen enlarged and inflamed; serum in cavity of chest. Otherwise as in cases Nos. 1 and 2.

One calf taken on the 18th and two on the 20th; symptoms and treatment same as those on 12th; took feed on 23d, and on 23d seemed well; have continued so to present time.

There having been no symptoms of disease among stock on any other part of plantation, we had hoped to confine it to the one lot, but on the 29th of September a yearling heifer in a lot of 23 head, nearly a mile away, was found to be ailing; brought her to hospital; urine dark wine color; treated as above; on 30th developed nervous symptoms; added a diuretic and antispasmodic; October 1st, worse; on 4th she died: *post mortem* revealed serum within membrane of brain; bladder filled with fluid color of porter; liver, gall, kidneys, and spleen all involved; contents of gall bladder thick and black as tar; on opening thorax found the cavity filled with blood, the anterior ventricle of the heart soft and flabby, and nearly the whole surface discolored; the posterior ventricle of proper color and consistency, but not a drop of blood in the organ; left lung adherent to pleura; right lung sloughing and adhering to sternum; a cyst containing pus, and a portion of right lung hepatized; serum visible on incision into any organ in the trunk.

This concludes my observations to this date. Summary: Two died without treatment, three under treatment, while six yielded to the medicine and care. One now in hospital shows strong indications of a fatal termination; another with unusual looseness of the bowels and a low condition, otherwise not *visibly* affected.

All the cattle in the pasture in which disease originated, and all others that have died in this section, so far as I know, are covered with ticks. The yearlings that died from the other pasture of 23 head had no ticks, nor have any of the cattle in the other fields on this place.

Our stock consists of 40 milch cows, about half native, balance graded Shorthorns; 70 head of one, two, and three-year-old graded Shorthorns; twenty calves; all of which have been bred on the place or brought here when very young. No cattle have been purchased for the place within the last five years. I have considered this somewhat lengthy description of my individual experience necessary, that you may determine the nature of the disease. I am of the opinion it is epizootic in its nature, from the fact that cattle isolated and at a distance from the public roads are not affected, while nearly all deaths are traceable to contact with oxen, which are used almost exclusively by the colored people, who, although not *heavy* losers, will be great sufferers if the plague is not arrested.

Have just returned from visiting and treating a fine steer owned by a poor renter, whose tobacco and corn must suffer if his team is broken up. Have heard of 60 deaths within 8 miles, and learn the scourge has been more severe south and east of us. My stock has occupied my entire time, and I find it impossible to learn much of the character of the disease from the natives, all answer to inquiries being "got the 'stemper,'" and the only symptom "won't eat." All die, as far as heard from, without exception. No *post mortem* is attempted, except to open the maw, which is said to be hard and dry, although I have found none yet in that condition.

I may be allowed to state that I have had a long experience with cattle, and never felt so apprehensive and powerless as *now*. I sincerely hope the Department may be able and the Government *willing* to afford us relief.

Very respectfully,

W. P. DYE.

CARTHAGE, MO., October 9, 1882.

Hon. GEORGE B. LORING,

Commissioner of Agriculture:

SIR: I have just read with much interest your special report No. 50, in regard to Texas fever of cattle. We are much troubled in this vicinity with the loss of cattle from what I consider the same cause. I have just (within a month) lost 9 valuable milch cows out of my herd of 40, and I think fully one-half the cows in this city have died during the season. I have no cases of sickness now, but many are dying for others. I wish to know if it would not be possible to have a veterinary surgeon come here and inquire and examine into this matter. Cattle die more or less here every year. Last summer I lost 14, and the year before 6 milch cows.

Difference of opinion exists as to the cause of the disease. Many attribute it to the mineral in the water, others to alkali, and others to poisonous vegetation, &c., &c.

I wish to procure all the information possible upon this subject, and if you know anything that will assist me I would be pleased to have it.

Yours truly,

L. E. STEINMETZ.

VEVAY, IND., October 19, 1882.

Hon. GEORGE B. LORING,

Commissioner of Agriculture:

SIR: The disease described as splenic or Texas fever has broken out in this county on the farm of Mr. Perry K. Cotton, near Moorefield. Every effort is being made, according to Department suggestions, to prevent the spread of the disease. Mr.

Cotton has written me a detailed report, which I inclose. It is proper to remark here that there have been no other cases throughout the county.

Very respectfully,

JAMES B. MCCRELLIS,
Statistical Correspondent.

MOOREFIELD, IND., October 17, 1882.

Mr. JAMES B. MCCRELLIS:

SIR: At your request would say, that I received from the Bourbon pens, at Louisville, Ky., 49 cattle on the following dates: April 6, 1882, 16 steers; April 11, 1882, 21 steers; June 27, 1882, 12 steers. I know nothing as to where they came from when brought to Louisville. They are neither Texan nor Cherokee cattle. We put them in pastures with native cattle. About the 1st of August found one of the natives dead; did not know it had been sick. One week after found several sick. About one-half of those afflicted died. The disease has appeared three different times with usually an interval of a week or ten days between. The first indications of disease is a staring coat; a disposition to isolate themselves from the herd; visible mucous membranes very pale; muzzle dry; horns and extremities cold; temperature not taken; breathing not unnatural until nearing death; not costive; when nearing death they seem to suffer greatly. Two or three have been so severely handled as to suffer from convulsions. A *post mortem* revealed the body to be almost bloodless. Liver in a healthy condition; lungs in a normal condition. The spleen appeared to be the only part much affected. It is usually three or four times its usual size. The blood in the spleen is almost black, and the whole organ in a broken-down condition. Kidneys in apparently a normal condition. Bladder well filled with a dark wine-colored fluid with albumen.

No fresh outbreak of the disease has occurred for about two weeks. Eighteen animals in all have died. The disease is certainly splenic or Texas fever.

PERRY K. COTTON.

DANVILLE, KY., November 6, 1882.

Hon. GEORGE B. LORING,

Commissioner of Agriculture:

SIR: Texas fever has been raging here to an alarming extent in several counties of Southern and Central Kentucky. Have you the power to have it investigated and stopped or experimented with? If so, will you either give me that power or send some one here to do so?

Truly, &c.

GEO. C. FAVILLE.

CHESTER, ILL., November 30, 1882.

Hon. GEO. B. LORING,

Commissioner of Agriculture:

SIR: I have the honor to make the following report of cattle disease (Texas fever) as it occurred at this place the last two months:

Two lots of cattle were received at the penitentiary grounds, one on September 19, 1882, containing 22 head, from Pinckneyville, the other on the 23d of September, containing 28 head, said to be from Missouri. They were all put together and selections were made for butchering of those in good plight, indiscriminately, until the 9th of October, at which time it was discovered that one cow and one steer were not well. The cow died on the 9th October in the afternoon. From this time to the 21st of October 16 head of the cattle died. I did not learn of the existence of the disease until the 10th or 11th of the month. I then began to give the matter attention, making *post-mortem* examinations, and in all cases examined I found the same condition of spleen and liver; in other respects there were variations. Two animals were examined with considerable care.

Cow No. 3 died October 21 at 3 o'clock p. m. The *post mortem* was made October 22 at 10 o'clock a. m. The animal had been skinned about one hour before the ex-

amination. No *post-mortem* changes were apparent. The body seem especially bloodless, considering that she had not been bled. The chest was opened on the left side by removing the entire ribs. There was very little serum in the chest cavities. The lungs were more or less congested, the lower portions of each sufficiently condensed to sink in water. There existed interlobular emphysema very strongly marked in the middle and upper portions. The lower portion of the right was slightly adherent. That may have been an old adhesion; could not see it well. The trachea contained thick material resembling pus, from the larynx to the bronchial tubes, and in the latter there was reddish frothy mucus. It will be borne in mind that the cow had been drenched. The heart was literally gorged with coagula in all its cavities. The portions of the clots near the apex were of a dark, translucent appearance, evidently coagulated fibrin. The other was dark and firm blood clots.

There was no peritonitis. The liver was considerably larger than normal, and one section was very yellow. The gall bladder contained 45 ounces of bile, the specific gravity of which was 1020. It was of a deep yellow color and of greatly increased consistency. The spleen was enlarged to about three times its normal size, and weighed six pounds; was very dark in color, and easily broken down. When cut, it had, internally, a dark color, a little tinged with red, nearly a plum-juice color, and it was easily broken down under pressure of the finger, apparently almost a structureless mass. The kidneys were abnormally large, the right one especially so, and contained quite a cyst, holding two or three drachms of a serous looking fluid. The urinary bladder contained 126 ounces of urine by measure, specific gravity 1017. It was nearly normal in color, contained more than a trace of albumen, but not enough to compute by proportion.

Microscopical examination showed a few flakes of epithelial scales—no casts. The rectum was moderately filled with lumps of fecal matter covered with what appeared to be mucus, and the gut was strongly congested at its lower portion. This cow had been sick about ten or twelve days, having gotten better and then relapsed.

Cow No. 4 died in the night of October 21 or morning of 22d. She had been sick about eight days. The *post mortem* was made at 11½ a. m. October 22. She had been skinned about an hour. Rather bloodless in appearance; external fat deep yellow. No indication of *post-mortem* changes or decomposition. Chest was opened by the removal of ribs from the left side. There was quite a considerable amount of serum in the chest cavity. Lungs congested in the lower portions, but not so much so as in cow No. 3, though the lower portions would nearly sink in water. This cow had also been drenched. The spleen and liver were in much the same condition as in the other animal. The gall bladder was moderately filled with inspissated gall, so thick it would scarcely run, and it had the appearance of containing granular material. It was very dark with a greenish tinge. It was too thick to take its specific gravity conveniently. The urine was of a light reddish tinge with numerous flakes in it. In quantity, 78½ ounces. Specific gravity, 1015. It contained rather a stronger trace of albumen than the other, and under the microscope more of the epithelium was shown, but no casts of the uriniferous tubules. The appearances and *post-mortem* changes were those found in the Texas cattle fever, such as have been reported during the past ten years.

The remainder of the cattle gradually recovered, and after about a month's time from the death of the last, October 21, they were all sold off in one lot.

Permit me to acknowledge the receipt of reports from your Department, as late as special report No. 50.

I have the honor to be, very respectfully,

H. Z. GILL, M. D.

There were many other outbreaks of this fever in different localities, causing such heavy losses that the Department deemed it necessary to issue a special bulletin (Special Report No. 50), giving information as

to the means of its dissemination and pointing out measures for its control. The following paper from the pen of Dr. D. E. Salmon was widely circulated in the above report, but it is considered of sufficient value to entitle it to a still wider circulation in this volume :

THE DISSEMINATION OF TEXAS FEVER, AND HOW TO CONTROL IT.

Hon. GEO. B. LORING,

Commissioner of Agriculture :

SIR: In accordance with your directions, I respectfully submit the following considerations in regard to the dissemination and prevention of splenic or Texas cattle fever, which have been suggested by Dr. Miller's report of outbreaks in Ohio and West Virginia.

In the first place, I would call attention to the fact that this is a disease with which the veterinary profession is not very familiar. The outbreaks, though frequent, have generally occurred in the Southern, Southwestern, or Western States, in localities where, until very recently, there have been no veterinarians; and when these have happened at the North, the course of the disease has been so rapid that the professional man has had little opportunity to study its peculiarities before it had exhausted itself by the death of all the susceptible animals exposed to it. As a consequence, members of the profession at large have not those clear ideas in regard to its origin, its dissemination, and the best means of suppressing it which we should expect had they been for a longer time in contact with it. And for this reason, while the veterinarian who has had little experience with it may recognize it at once by the symptoms and *post-mortem* appearances, we cannot expect him to be as reliable a guide for the measures to be adopted in controlling the plague as would be the case had he watched and studied it for years.

The quarantine of sick animals and infected grounds is so generally applicable to infectious diseases as a class, and has been our chief reliance for so long, that it is not surprising to find it advocated for an affection like Texas fever, which has the same general characters, even though there are peculiarities in regard to it which may render this measure, as generally applied, inexpedient or plainly objectionable. We must not forget, however, that the owners of sick cattle and infected grounds have certain rights as well as the public at large, and that while it is our duty to assist the public in protecting itself by advocating such regulations as are necessary for accomplishing this end, it is equally our duty to be certain that the regulations proposed are necessary, and that they do not bear unduly upon the already suffering parties, or restrict the business of the community further than is absolutely essential.

To make it plain, therefore, what steps should be taken by the local authorities in the case of such outbreaks of Texas fever as have recently occurred in Ohio, West Virginia, New York, and other States, where there is no permanent infection with the germs of this disease, it is necessary to enter into some details concerning the spread of this infection.

HOW IS TEXAS FEVER DISSEMINATED ?

Those who have had much experience with Texas fever, who have watched the outbreaks in the Northern States, who have followed its ravages along the border line of the permanently infected district, and have studied it in its native haunts, are practically unanimous in their conclusion that the Southern cattle may only be separated from susceptible ones by a board fence, or that they may even occupy the same stable for an indefinite time, without causing the least appearance of disease. There is equal unanimity in the conclusion that if these susceptible natives are placed upon grounds, even though these be roads, yards, or commons to which the Southern cattle have had access within two, four, or six months, they are liable to contract the disease in its most virulent form. On the other hand, it is believed that the sick natives have never conveyed the disease to other susceptible animals either directly by contact or

indirectly through the atmosphere, or by infecting the pastures on which they run. Having frequently seen sick animals in the same lots with susceptible well ones, and having injected considerable quantities of the blood of recently dead animals beneath the skin of susceptible cattle without any transmission of the disease, I am perfectly convinced that this opinion is correct, and that the sick native animals are in no sense a source of the infection.

In regard to the danger of the disease being spread from the dead carcass we cannot be so certain. My investigations indicate that the germs of the disease exist in the spleen and liver, and it would not be unreasonable to suppose that these organs carried about by dogs or other animals might in certain cases be the means of infecting other pastures. We do not know, however, that this has ever occurred.

The real danger, then, exists in the pastures or other grounds over which Southern cattle, whether sick or well, have traveled, while the sick natives are harmless.

The Southern cattle which convey the infection do not, as a rule, contract the disease, but this rule is not without exceptions. The germs of the disease are within their bodies, probably in their digestive organs, possibly, also, in the liver and spleen; and though when in vigorous condition they are insusceptible to the influence of these germs, when exhausted by the hardships of travel they frequently succumb to them. There is, consequently, a distinction to be drawn between the sick native animals and those from the South which have sickened; the former do not infect pastures, the latter in all probability do infect them.

We must not expect to find these facts accepted by all who observe this disease, however; on the contrary, they are frequently contested, and nowhere more emphatically than along the border line of the permanently infected district, where the disease is most common, and where it is most important that they should be understood. The experienced sanitarian will not be surprised at this; he knows that the same is true with all communicable diseases, and that it is precisely where these are most common that there is the greatest doubt as to the manner of their origin and dissemination. Every unbiased man is ready to admit, for instance, that pleuro-pneumonia never occurs in this country unless it is contracted from a previously sick animal, because the disease was never known here before it was imported from Europe, and because it never occurs now beyond the area obviously infected except by contagion. And yet, when we consult the people of the infected districts, we find that many are emphatic in their assertions that the disease occurs spontaneously as the result of certain conditions of the atmosphere or food, and that it does not arise in all cases from contagion. The reason is very plain. The contagion is so generally disseminated that it is impossible to trace a large number of the cases to their origin, and they are, consequently, accepted as spontaneous. If we go to France, we find a still more general acceptance of the opinion that this disease arises spontaneously, and this is even shared by many members of the veterinary profession. But as the disease is becoming more circumscribed, and as greater efforts are being made to trace the origin of the outbreaks, the profession, at least, is gradually becoming convinced that it only arises by contagion from pre-existing cases.

Along the border line for miles beyond the district permanently infected with Texas fever, every road and common is infected in early spring by the continuous movement of cattle, and it is not surprising that many outbreaks of the disease can never be traced to their source. So, too, in many cases where Southern cattle are carried farther north, an unsuspected road or common is infected, and it long remains an unsolved mystery how the native cattle contracted the disease. Again, people frequently forget that foreign cattle were on their roads and pastures, it may be three to five months before the outbreak of the disease; they are almost as likely to forget that their own cattle have been on certain roads or pastures within three to six weeks; they even forget that they have purchased cattle lately enough to cause the infection, and in all these cases they will assert most positively that their cattle have had no opportunity to contract the plague in the ordinary way, and that, consequently, it has been carried through the air or has originated spontaneously. A careful investiga-

tion, however, generally discovers the infected grounds and the cattle which have caused them, and then everyone is surprised that that had not been thought of before.

I mention these facts to show the necessity of the greatest caution in accepting explanations of outbreaks as valid which apparently contradict the great mass of facts bearing on this or any other contagious disease.

Turning now to the outbreak in Ohio investigated by Dr. Miller, we find that both lots of cattle were purchased in the Saint Louis stock-yards, and that, at least, one lot was represented to have been purchased in Southwestern Missouri. Lot No. 1 may have been infected when purchased in Saint Louis, or, what is equally probable, it may have been a mixed lot of animals—one or more coming from an infected district, while the others were from farther north and susceptible to the disease. They could not have all been from an infected district, for in that case there would have been no deaths except during the first week after their arrival, when exhausted by the journey. If none were from the infected district, and the infection was contracted from grounds on which they had been yarded during the journey, then the pasture on which they died would not have been infected. There seems to be no data whatever for deciding this point.

In regard to the second lot, the impression conveyed by the report is that they were infected by driving lot No. 1 along the road adjoining their pasture. Considering that the infection has never been positively known to have crossed a fence, I think a more satisfactory explanation is possible under the circumstances. This lot, like the other, being purchased in the Saint Louis stock-yard, was very likely composed of some animals from an infected district, and others which were from uninfected localities and consequently susceptible. Indeed, if all had been susceptible we should have expected that a larger proportion would have contracted the disease. The fact that they did not sicken until after the first cases occurred in the other lot does not bear against this view, since it is a matter of common observation that the disease frequently does not occur until a certain season of the year, no matter how long the pasture has been infected. Thus, cattle taken upon infected pastures in early spring do not, as a rule, show any symptoms of the disease until August, while those put upon the same pasture in July very often sicken as soon. So that, if each lot had contained one or more animals capable of infecting the pastures, it is not surprising that the disease occurred about the same time in both cases, though one had been upon its pasture a month longer than the other.

In West Virginia the Southern cattle seem to have been divided among a number of farmers, and consequently, there must have been abundant opportunity for the infection of roads, commons, and pastures. While, therefore, the owners of some of the diseased native cattle may have believed that these had not been upon infected grounds, it is altogether probable that, as has happened in so many other cases, either the owner was not cognizant of all the wanderings of his cattle for the preceding three or four weeks, or that grounds which were not suspected had been contaminated by the foreign animals.

I conclude, therefore, that there is not sufficient evidence in either of these cases to show that Texas fever is disseminated in any other way than by means of the infected roads, yards, or pastures; and I insist more particularly upon this point because it furnishes the chief indications for the measures to be adopted in suppressing such outbreaks.

WHAT MEASURES SHOULD BE ADOPTED BY LOCAL AUTHORITIES FOR SUPPRESSING TEXAS FEVER.

When such outbreaks occur as have recently become so frequent in the Northern States, by the introduction of Southern cattle, it at once becomes a matter for serious consideration with the local authorities to decide what regulations should be enforced to protect the neighboring cattle from infection. The extreme virulence of the disease, and the suddenness with which large herds are almost exterminated, makes it seem imperative that some very stringent measures should be adopted at once. The alarm is as unexpected as though a fire had broken out, and the emergency is so unprepared

for that not unfrequently the ill-considered regulations enforced result in more real loss to the community than would be caused by the disease itself.

From a consideration of the facts which I have gone over, however, it is very evident that it is unreasonable and unjust to compel the owner of the Southern cattle to do more than keep these animals securely fenced upon the infected pasture until after a killing frost, and to bury beyond the reach of dogs any that may die. Where the infected pasture adjoins a road or neighbor's field, on which there are susceptible animals, it may be advisable, for complete security, to build a second fence which would keep the dangerous cattle from coming within a rod of such road or field. But to go upon a man's premises and kill his animals, sick or well, and compel him to pay an exorbitant price for the slaughter and burying, as may be done in outbreaks of this disease by the laws of some States, is an outrage for which there is no justification in the characters of the affection.

The Southern cattle may infect pastures and roads, but there is not a particle of satisfactory evidence that they can disseminate the disease in any other way; and after the first really severe frost such grounds are no longer dangerous. If, therefore, these cattle are quarantined upon the infected pasture where they cannot come within a rod of other animals they can do no more harm. The sick native animals do not propagate the disease either directly or by means of pastures; they are consequently harmless, and it is questionable if the authorities should interfere with them, further than to prevent their sale for food while diseased.

Finally, it must be a very extensive outbreak which will justify restrictions upon the ordinary traffic in the native cattle of any township or county. If a farmer has one infected field, that certainly is no sufficient reason why he should not be allowed to market animals which have not been upon that field since it was infected; and it is even less reasonable for quarantining his neighbors. It is true that the roads may be infected and that cattle driven over them may be liable to contract the disease, but this is by no means certain, and if the owner chooses to take the risk he evidently has the right to do so, since he alone will suffer. If a road is known to be infected a notice should be posted, in the interest of the public, at the nearest cross-road in each direction, warning people of the danger of driving cattle over it; but the owner of susceptible cattle is in no danger while he keeps them from the grounds upon which those from the South have traveled, and he, consequently, needs no such unusual measures for his protection as is implied in a general quarantine of all bovine animals.

It is plain, however, that the proper remedy is beyond all local regulations; that it should prevent the infection of roads, commons, and pastures, by prohibiting the introduction of cattle from the district permanently infected with Texas fever. This is a matter of infinite importance to the country at large, but it is also one that has been and will be attended with unusual difficulties and that will require the greatest wisdom and experience to perfect its many details. It is doubtful if it can ever be accomplished by the States individually, and it would seem that some way must soon be devised by which the National Government can draw a line from the Atlantic to the Rocky Mountains, across which all movement of cattle can be definitely controlled. If we are at present in the absurd predicament that the States are unable to enforce effective legislation because this is interfering with the prerogatives of the nation, and that the latter can do nothing because this would be violating the rights of the States, there certainly can be no good reason, in this practical age and with our present standard of intelligence, why we should not be able to extricate ourselves whenever we are satisfied that this is essential to our interests.

In cases where native cattle are upon infected pastures the *owner* can do something towards checking the progress of the disease among his stock by removing them at once to an uncontaminated field.

Respectfully submitted.

D. E. SALMON, D. V. M.

ASHEVILLE, N. C., Sept. 15, 1882.

DISEASES OF SHEEP IN TEXAS.

REPORT OF DR. H. J. DETMERS.

Hon. GEO. B. LORING,

Commissioner of Agriculture :

SIR: In the following pages I have the honor of laying before you my report on the diseases of sheep in Texas, or more particularly in the sheep-raising portions of Western Texas. Before I enter into the details of my investigations, allow me to say a few words in general. Western Texas, or those portions of the State in which wool-growing has developed into an important industry, is well adapted to sheep-raising; all conditions necessary are existing, and malignant diseases of sheep are perhaps less frequent than in any other part of the United States, provided the sheep are properly kept and taken care of, and no attempts are made to raise and to keep them on lands not adapted to sheep-raising. Sheep, but wool-sheep in particular, in order to be healthy, require a dry and salubrious climate, and a soil not inclined to be muddy or wet. Land full of sloughs, pools of stagnant water, "hog-wallows" or "buffalo-wallows," or land that is low and level, constantly wet or soggy, or full of so-called "swales," &c., should never be used for sheep-raising. But as it is, many former lawyers, preachers, merchants, mechanics, &c., who know nothing about the requirements of a sheep, come to Western Texas—some of them for their health—and engage in sheep-raising, because the land is cheap and the business profitable. It is mostly these men who complain about losses caused either by a bad selection of locality, or by bad management and want of proper care, while experienced flock-masters, who understand their business, and superintend their flocks in person, have very little cause to complain, and hardly ever do complain about losses from disease. The only real drawback to sheep-raising in Texas—I mean Western Texas, as Eastern Texas is not adapted to wool-growing—is caused by the blow-fly, and perhaps, also, though in much less a degree, by the gad-fly, *Æstrus ovis*. The former, particularly in the summer, lays its eggs in any little wound or sore that is exposed, produced either accidentally or by marking, docking, castrating, &c., while the larvæ of the latter, as is well known, often cause serious trouble in the frontal and maxillary sinuses and in the nasal cavities and ethmoid bones. Still, an attentive flock-master suffers but very little from the causes just named. He has learned to destroy the larvæ of the blow-fly, the so-called "screw-worms." He carefully watches his sheep, and wherever the "screw-worm" makes its appearance he is at hand with his carbolic salve, his

chrysalis ointment, &c., and destroys it. Besides, he avoids as much as possible to wound his sheep during the fly season, and performs all necessary operations, such as marking, docking, and castrating, either in the spring or in the fall. He also sees to it that no lambs are born in the summer, because he knows the blow-fly will immediately attack the naval of the new-born animal. As to the larvæ of the gad-fly, his task is a little more, but not so very difficult either. In the first place, he takes care that the nostrils of his sheep during the summer months are well anointed with tar or other substances which are objectionable to the gad-fly, prevent it from depositing its eggs, or make it impossible for the latter to reach their destination. Further, knowing that the larvæ of the gad-fly are discharged from the nostrils of the sheep late in the fall and in the winter, that once discharged they soon burrow into the ground, where they undergo their change into pupæ, and that the pupæ develop into flies in the spring and fore part of summer, according to their age; also that the gad-fly arrives at sexual maturity and commences to deposit its eggs in June, July, or August, as the case may be, and that hardly any more eggs will be deposited after September, an experienced flock-master will see to it that his winter range is not occupied by his sheep from June to September, and *vice versa*.

Among the diseases most damaging to the wool-growing industry, scab, of course, deserves the first place, but the Texas flock-masters have learned to manage that too. While formerly scabby flocks were the rule, at present they are an exception. Nay, more, in the whole southwestern part of Texas but very few flocks can be found affected with that disease, because every flock-master is on his guard. If he has the least suspicion he at once resorts to dipping, and once his flock is clean he knows that it will remain clean as long as no scab is imported. He therefore endeavors to protect his range against scabby droves of sheep coming from other places or other States, and the Wool-Grower's Association of Texas is making efforts to induce the legislature now in session to pass a very strict and effective scab law, such a one as will make any importation of scabby sheep next to impossible. Sheep driven into Texas through New Mexico from California are nearly all affected with scab, hence such a law will be very beneficial. The wool-growers of Texas desire a law which compels all imported sheep, but particularly such as come in droves, to be "dipped" twice before they are allowed to enter the State. If such a law is passed and properly executed, if the dipping is done in a thorough manner, if all other necessary precautions well known to every flock-master are observed, and the second "dipping" follows the first not earlier than the sixth nor later than the tenth day, there is a prospect that scab will soon be a very rare disease in Texas.

The most destructive disease of sheep undoubtedly is anthrax. It occurs, however, only in certain localities, and generally in those which

are not adapted to sheep-raising, or in which the owners of the sheep are guilty of gross negligence. About anthrax of sheep I shall have a little more to say further on.

The only other diseases of sheep that deserve special attention as far as Texas is concerned are those which are caused by entozoa or worms. The fluke, *Distomum hepaticum*, which has its seat in the bile-ducts and in the gall-bladder, and is a curse to sheep-raising in many parts of Europe, occurs frequently only on the low and level lands near the Gulf, consequently on lands much better qualified for cattle than for sheep. In comparatively rare instances the fluke-worm, it is true, also occurs in other parts of Texas; for instance as I was recently informed by Mr. C. Real, in Kerr County; but there, it seems, it owes its presence to recent importations, and will probably not gain a permanent foothold. Although the steadily growing trade in sheep, and particularly the constantly increasing importation of finer sheep from other States, will undoubtedly be followed by a more frequent occurrence of the fluke-worm, it may be safely predicted that in Texas, or rather in those parts of Texas better adapted to sheep-raising than to anything else, the fluke will not at any time cause any serious losses, because all those parts of the State which are more suitable for sheep than for cattle are on the whole too dry to afford favorable conditions for the preservation and propagation of that entozoon. In certain localities in Texas, viz., in the vicinity of Austin and of Fort Worth, and probably in some other districts, occurs quite often a tape-worm, *Tenia expansa*, particularly in lambs; but it seems that the losses caused by this worm are not very serious. *Tenia expansa* occurs in the small intestines of sheep, but principally of lambs, and is sometimes found also in the small intestines of other ruminating animals, such as goats and cattle. It grows sometimes to an enormous length. Its head is very small and without hooks, and its joints or proglottides close to the head are also very small, while those further back gradually increase in size, so that the largest are very large and almost square. It is said that specimens occur which are one hundred feet long, and have proglottides, the largest ones, one inch in width. (Those I had an opportunity of seeing I estimated to be from twenty to thirty feet long, and their largest proglottides or joints appeared to be from half an inch to five-eighths of an inch in width.)

Of cyst-worms more than one species have been found at the various *post-mortem* examinations, and if I may judge from what I have seen, at least two species, namely, *Echinococcus veterinorum* and *Cysticercus pisiformis* are of rather frequent occurrence not only in sheep but also in rabbits. Whether or not they ever cause serious losses I am unable to state, because in nearly all cases in which I found them I also found other entozoa and other morbid changes evidently not caused by those cyst-worms. *Cysticercus cerebralis*, a cyst-worm which has its seat usually in the brain and occasionally in the spinal cord, and which

occurs quite often in certain parts of Europe, has never been met with at any *post-mortem* examination, and I believe I am correct when I say its existence is almost unknown in Texas, and it is doubtful whether it ever has been found in that State.

As to the other cyst-worms mentioned (*Echinococcus veterinorum* and *Cysticercus pisiformis*) I shall have more to say below.

Far more damage than by all those entozoa just mentioned is done by two small, thread-like worms belonging to the genus *Strongylus*. The one, *Strongylus filaria*, a whitish, thread-like worm from 1 to 3½ inches long, is met with quite often, and sometimes in large numbers, in the bronchial tubes of sheep, but particularly in those of lambs, and cause, according to my observation, the most losses; is also, on account of having its seat in the lungs and not in the stomach or intestines, the most difficult to destroy. The other one, *Strongylus contortus*, a reddish-looking worm, is a trifle smaller, and occurs sometimes in immense numbers, but more frequently as a few specimens in the fourth stomach of lambs and of sheep, principally of the former. If present in large numbers it causes gastric disorders, more or less rapid emaciation, and anæmia, which not seldom terminate in death. The Texas flock-master calls the disease thus produced

LOMBRIZ.

Lombriz is a Spanish word, and means "worm." In Texas it is often indiscriminately applied to all cachectic or anæmic diseases of sheep, no matter whether caused by the presence of *Strongylus contortus* ("lombriz worm"), by other entozoa, such as *Strongylus filaria* in the lungs, cyst-worms in various tissues, and *Tenia expansa* in the small intestines, or by something entirely different. Still, the more experienced flock-masters make a distinction, and limit the term "*lombriz*" to the damage done by *Strongylus contortus* in the fourth stomach. In the following pages I shall use it in the same sense.

In your favor of March 7, 1882, you directed me to make an investigation of the diseases (of sheep) mentioned in an inclosed letter written to you by Mr. David M. Clarkson, of Brackett, Kinney County, Texas. The diseases mentioned by Mr. Clarkson in said letter are scab, *lombriz*, and foot-rot. As scab is a disease which is well known to and rationally treated by every flock-master in Texas who has had any experience at all, and as foot-rot is practically an unknown disease in Texas—at least I have not been able to find or to hear of a case that originated within the borders of that State—I directed my principal attention to so-called *lombriz* and kindred diseases (worm epizootics). In your favor of the 7th of March you requested me to correspond with Mr. Clarkson. I at once complied with this request, but had to wait some time for an answer. When it arrived it was worded so as to cause me to conclude that *lombriz* was prevailing and causing great losses in Mr. Clarkson's flock of sheep. I received Mr. Clarkson's letter on March

31, and as soon as circumstances permitted, April 3, I went to his ranch, about 12 miles from Brackett, in Kinney County. I arrived there on April 4, and visited not only Mr. Clarkson's ranch but also several others, among them the ranch of Mr. Newell, vice-president of the Texas Wool-Growers' Association (April 5, 6, 7, and 8). My trip, however, was productive of very small results, for no *lombriz* existed or could be found in any of those herds visited. Neither Mr. Clarkson nor his neighbors ever had a case in their flocks. Mr. Newell, it is true, had a few cases and suffered slight losses in 1881 in a newly-purchased flock of sheep that had not been raised in that part of the country. Neither could anybody give me any reliable information as to the nature of the disease. Of Mr. Newell I learned that the term "*lombriz*" is applied to a disease of sheep caused by a large number of small worms found in the fourth stomach, and that very likely no fatal cases would occur anywhere before June or July. As nobody could give me any definite description of the worms in question, notwithstanding I applied for information to quite a number of gentlemen supposed to be familiar with the subject, I had to wait for further developments. Finally, in May, through the kindness of Mr. John Wickeland, who has a sheep-ranch 14 miles from San Antonio, on the Martinez, I received specimens of the so-called *lombriz* worm, which had been taken from a sheep in August, 1881, and preserved in alcohol. From these tolerably well-preserved worms I soon recognized *Strongylus contortus*, and consequently knew what I had to deal with. Having taken pains to inform the sheepmen of Texas, through their papers, of my mission, letters and telegrams inviting me to come to the ranches and to investigate "*lombriz*" in a short time commenced to arrive. By complying as much as possible with these invitations I soon learned that the term "*lombriz*" is by many sheepmen, even by many who have been a long time engaged in sheep-raising, indiscriminately applied to every chronic disease of sheep which the shepherd—usually a very ignorant Mexican, who if a sheep dies hardly ever makes a *post-mortem* examination—is unable to diagnose. So it often happens that in many a flock of sheep the term "*lombriz*" covers all diseases that occur, and particularly all that prove fatal.

To relate all my disappointments in detail would be very tedious and of no practical use. I will, therefore, only mention a few instances. On May 17 I received a letter from the Texas Wool office, of which the following is a literal copy:

Dr. DETMERS:

DEAR SIR: I to-day received a telegram from Mr. Nuessel, of Uvalde, saying that he has *lombriz* among his sheep, several lambs dying every day. I would like to have you visit him.

Yours, respectfully,

L. A. HEIL, *Texas Wool.*

I at once went to Uvalde, and found, when I arrived at Mr. Nuessel's ranch, 5 or 6 miles north of Uvalde, that out of a large flock of sheep—6,000 or 7,000—only a few lambs—11 or 13, I believe—had died, and that no *post-mortem* examination had been made. It was difficult to find a sick lamb. Finally, an emaciated and rather poor-looking lamb was pointed out to me by Mr. Nuessel and his shepherd as being affected with *lombriz*. After it had been killed for a *post-mortem* examination nothing of any importance could be found except a quite extensive degeneration of the mesenteric glands, a general anæmic condition, and some serum in the thoracic and abdominal cavities; or, in other words, what might be called a scrofulous condition and its natural consequences. Most of Uvalde County, and particularly Mr. Nuessel's ranch, is very dry, and therefore, as will be shown below, not offering favorable conditions for the preservation of worm brood, consequently is not apt to harbor much *lombriz*.

The first case of real *lombriz* I had an opportunity of seeing was at Mr. David Brown's ranch, in Atascosa County, about 16 miles south of Pleasanton. It was from May 24 to May 28. Mr. Brown, one of the oldest and most experienced flock-masters in Texas, and well acquainted with the disease in question, informed me that not much *lombriz* might be expected till the middle or latter part of June, except in its incipient stage, but, with his well-known generosity, pointed out a two-year-old imported buck as being affected with that disease, and offered to kill the animal for a *post-mortem* examination. His offer, of course, was accepted. The buck was killed by bleeding, and quite a large number of small—not fully developed—*Strongyli contorti*, imbedded with their heads in the mucous membrane, were found in the fourth stomach. A subsequent microscopic examination proved the same, or at any rate a large majority of them, to be young worms or worms not yet arrived at sexual maturity; still, the males and females, in a proportion of about one male to five females, could be plainly distinguished. In the same animal, however, I discovered another disease, in my opinion more serious, as will be explained below, than the presence of the entozoa in question in the fourth stomach. On opening the chest, taking out the lungs, and then opening the bronchial tubes, I found in the latter, but particularly in those of the posterior portions of the lungs, numerous specimens of another *Strongylus*, which is known as *Strongylus filaria*. It is a little larger than the "*lombriz*" worm, and also of a different color, therefore easily distinguished. As to these two worms of the genus *Strongylus* the differences are as follows: *Strongylus filaria* (Rud.), which occurs in the bronchial tubes of lambs and sheep, sometimes in very large numbers, is a small, round, thread-like worm, tapering at both ends, but more gradually anteriorly than posteriorly. Its head is small, with mouth situated at the end, and its color is from a milk-white to a slightly dirty-looking white. The male worm measures in length from 1 to 1½ inches, while the female is twice as long and not seldom measures

fully $3\frac{1}{2}$ inches. Its thickness is about one thirty-sixth of an inch, or a little over two-thirds of a millimeter. The external sexual organs are situated at the posterior third of the body. *Strongylus contortus* (Rud.) is also a small, thin, thread-like worm, tapering toward both ends, but most toward the head, which is very small, and still smaller or more pointed than that of *Strongylus filaria*.* The color of both the male and female worm while alive is more or less reddish or brownish, and the female is distinguished by its contorted appearance. The male worm, which is very thin, measures hardly an inch in length, while a full-grown female is about 2 inches long and one forty-eighth of an inch, or a trifle over half a millimeter, thick. The external genital organs of the female worm are situated nearly half an inch anterior to the posterior end of the body, or in the posterior third part of the same, and protected by a valve-like appendice, and the genital organs of the male worm are situated at the hind end of the body and there inclosed by two broad claw-like appendices, which during the act of copulation serve to embrace the body of the female. As a good many worms after they have arrived at sexual maturity can be found in the act of copulation, it must be concluded that that act occupies a considerable length of time. The ovaries of the female are very long, and appear in the fully developed worm like corkscrew-shaped bands winding around the intestinal canal, and give the female worm its peculiar contorted appearance. Mr. John Wickeland, who examined some female worms with a low-power microscope, compared their appearance with that of a barber-post. It may just as well be stated right here that the female worms of both species, of *Strongylus contortus* as well as of *Strongylus filaria*, produce innumerable eggs, which develop living embryos while yet in the ovaries, and that the mother worm, as soon as these embryos have reached a certain stage of development, prepares to leave its place and is discharged, *Strongylus filaria* by coughing and sneezing, and *Strongylus contortus* with the excrements of the sheep. Once discharged, the old worm soon dies and decomposes—*Strongylus contortus*, it seems, dies soon after it leaves the fourth stomach and before it is discharged—and then the embryonic worms, still inclosed in and protected by the shell of the oblong egg, become free, and will live, if a certain degree of warmth and sufficient moisture are not wanting where they happen to be deposited, till a heavy rain carries them into a pond, a water-hole,

*There is an apparent discrepancy between my statements as to the size of *Strongylus contortus* and the size of my drawings. The difference, however, is only apparent, and can be readily explained. My statement gives the size of some large specimens (the largest that could be found) of fully matured worms while fresh and yet alive, while my drawings, made some months afterward, represent worms preserved in alcohol since September, and consequently somewhat shrunken. For instance, the drawings from the camera lucida of the female represents a large worm magnified precisely 42 diameters, and if the width is measured it will be found to be about 0.4 millimeters, or nearly $\frac{1}{2}$ of an inch, instead of $\frac{1}{8}$ of an inch, as stated in my report. In length the shrinkage is just as great, if not greater.

or a low, wet, and muddy place, in which they can pass their embryonic existence, either in muddy or stagnant water or on aquatic plants. If such water is used for drinking by lambs or sheep, or if the latter eat the aquatic plants, the embryonic worms reach their place of destination. Of course a great many embryonic worms, there can be no doubt, will perish, but as the eggs produced by each female worm are exceedingly numerous, and as the female worms outnumber the male worms about five times, there is no danger that the worm brood will become extinct, even if of every thousand embryos produced only one or a few reach a place—if *Strongylus contortus*, the fourth stomach, and if *Strongylus filaria*, the bronchial tubes of a lamb or a sheep—where they can develop to maturity. Hence, as long as flock-masters and shepherds allow their sheep and lambs to drink the stagnant water of pools, water-holes, hog-wallows, &c., and to eat the aquatic plants growing in or near such pools and water-holes, particularly if the range is infested with the brood of these worms, their propagation and future existence are abundantly secured. They will become extinct, though, if sheep, particularly from March to July, are not allowed to graze on land on which worm brood has been deposited, and are prevented from drinking stagnant water and from eating aquatic plants. The embryos of *Strongylus contortus*, once swallowed, soon reach the fourth stomach, where they burrow with their heads into the mucous membrane, while the embryos of *Strongylus filaria*, according to some authors (Colin), ascend from the stomach through the œsophagus, and then descend through the larynx and windpipe into the bronchial tubes. Whether such is the case, or whether the embryonic worms reach their destination in a more direct way, that is, directly from the throat through the larynx and windpipe, without first passing into the stomach, I am unable to decide.

At the *post-mortem* examination of Mr. Brown's two-year-old buck the other morbid changes present consisted in watery exudations in the thorax, in the abdominal cavity, and in the subcutaneous tissues between the lower jaws and at the throat, and of course must be considered as the consequence of the anæmic condition and general decline caused by the presence of the worms.

After this experience with so-called *lombriz* I visited several sheep ranches in different parts of the State, but in every instance was disappointed and found not what was promised and expected; on the contrary, it was always some sporadic disease as will occasionally occur in every flock of sheep.

On the ranch of Mr. William Gerfers, near the Cibolo, and about 22 miles from San Antonio, I had another opportunity, on June 9, 10, and 11, to observe real *lombriz*, as well in living animals as at *post mortem* examinations. In the living animal the symptoms are about the same as those caused by other intestinal entozoa, and consist in irregular digestion, more or less constipation followed by diarrhea, increasing

weakness, emaciation, paleness of the skin and of the visible mucous membranes, anemia and watery exudations, particularly in the subcutaneous tissues between the lower jaws and at the throat. At first the temperature is increased above normal, but immediately before a fatal termination the same often sinks to several degrees below normal. To give the exact symptoms of pure *lombriz* is rather difficult, because the lambs and sheep infested with *Strongylus contortus* in nearly every case also harbor other entozoa, such as *Strongylus filaria* in the lungs, various cyst-worms in other tissues, and besides are usually troubled with the larvæ of the gad-fly in the nasal cavities, ethmoid bones, and frontal and maxillary sinuses, which of course all contribute to complicate the symptoms.

One lamb and two muttons (wethers) were killed by bleeding in my presence. In the lamb I found tuberculosis in the lungs, extending to about one-third or more of the whole pulmonary tissue, and in the fourth stomach several but not very many worms of the *Strongylus contortus* kind. The ethmoid bones contained several larvæ of the gad-fly, *Æstras ovis*. No other morbid changes of any importance.

In the first wether I found "screw-worms" (larvæ of the blow-fly) in one foot, quite a number of *Strongylus filaria* in the bronchial tubes, a few *Strongylus contortus* in the fourth stomach, and numerous sterile or aborted cyst-worms on the serosa of the large intestines. Several of the mesenteric glands showed fatty degeneration.

In the second wether I found a few tubercles in the lungs, but no *Strongylus filaria* in the bronchial tubes, a few *Strongylus contortus* in the fourth stomach, and some aborted cyst-worms in the serosa of the large intestines. This second wether or mutton, which presented when alive the appearance of a perfectly healthy animal, and proved to be very fat, was butchered for meat, while the lamb and the first mutton or wether appeared to be sick when alive, and were killed for *post-mortem* examination. Both animals were in poor flesh, the lamb probably on account of its tuberculosis, and the wether undoubtedly had been damaged much more by the screw-worms (the larvæ of the blow-fly) than by anything else. The temperature of the lamb before it was killed proved to be 103°.8 F.

Mr. Gerfers, who is considered by Texas sheepmen as one of the most experienced flock-masters in Texas, believes that on a once infected sheep-range every sheep harbors at the proper season—from June to September—at least a few *lombriz* worms (*Strongylus contortus*) in the fourth stomach, but shows symptoms of disease—is damaged—only if those worms are very numerous, or if the constitution of the animal, from other causes, is a weak one. My own experience does not contradict his views; on the contrary, if it were expressed somewhat less sweepingly, if the word "nearly" were inserted before "every sheep," I would have to in-dorse it.

On June 13 and 14 I visited Mr. A. Real's sheep-ranch, 11 miles from

San Antonio. I found no diseased sheep, and range in a very good sanitary condition. Knowing that the so-called jack-rabbits, which are very numerous in Western Texas, often harbor large numbers of cyst-worms, similar in appearance or identical to those I so often found in sheep reported to be affected with *lombriz*, and that the corresponding tape-worms, *Tenia serrata* and *Tenia echinococcus*, occur in dogs, wolves, and coyotes, I made it a point to procure or to shoot one or more jack-rabbits on every sheep-range, because it appeared to me probable that the large number of rabbits essentially contributes to the propagation of the cyst-worms, and indirectly causes their frequent occurrence in sheep. The coyotes, wolves, and dogs catch the rabbits and eat them; the cyst-worms of the rabbits in that way pass into the stomach and intestines of the coyotes, wolves, and dogs, and develop to tape-worms, and these animals, thus getting tape-worms, deposit their excrement and with it the ripe or pregnant tape-worm-joints or proglottides, full of eggs, on the grass. As the proglottides once discharged will soon decompose, the eggs become free, and, possessing great vitality, they are not easily destroyed by external influences; have a very good chance of being picked up by sheep and rabbits that come along and eat the grass. At Mr. Real's range I killed one rabbit, and found it healthy, or free from cyst-worms. To avoid unnecessary repetitions, I may here state that wherever I found cyst-worms in sheep, I also found them in the rabbits, and *vice versa*; wherever the sheep were free, there the rabbits, too, had no cyst-worms. I therefore advocated an extermination of rabbits and coyotes, at least on all the ranges on which the sheep are infested with cyst-worms.

On June 14 and 15 I visited Mr. John Wickeland's ranch, on the Martinez, 14 miles from San Antonio. A fat mutton, apparently in perfect health, was killed for meat in my presence. The carcass was very fat. In the lungs, however, I found a few miliary tubercles, and in the fourth stomach a few *Strongylus contortus*. I just mention this case to show that a small number of *lombriz* worms do not seem to interfere with the well-being of a sheep. Before I returned to San Antonio I shot four rabbits, but found them free from cyst-worms.

On June 16 I received a telegram, and on June 17 a letter, both dated Uvalde, from a Mr. McLawrence, stating that his sheep were dying every day by the dozen. I at once answered by telegram and by letter that I would be at Uvalde on June 19, and asked Mr. McLawrence to have a conveyance for me at the depot to take me to his ranch. Accordingly I left San Antonio on the morning of the 19th and arrived at Uvalde in the afternoon of the same day, but found on my arrival that Mr. McLawrence, immediately after he had telegraphed to me, had left for his ranch, which I learned was 50 miles from Uvalde in the Frio Cañon, and can be reached only over a trail leading through an almost uninhabited country. Fortunately I became acquainted with Mr. J. V. Abrams, another flock-master, who lives on the Nueces, 8 miles from

Uvalde. Mr. Abrams took much interest in my mission, and offered to procure a team and to go with me to Mr. McLawrence's ranch. His kind offer, of course, was accepted. As I did not wish to lose much time, we left Uvalde on the evening of the 19th, and arrived the next morning at about 8 o'clock at Mr. McLawrence's residence in the Frio Cañon. Whether the location is in the northern part of Uvalde County or in the southern part of Bandera County I did not learn. On my arrival I soon ascertained that the sheep that died—in all about 150 to 160 head out of a flock of 1,100—had not died of *lombriz*, but of *anthrax*, and that they had continued to die at the rate of 12 to 15 a day as long as they occupied a certain range which contained two pools of stagnant water, but had stopped dying as soon as they were removed to another place. As the sheep had been removed on Saturday—I arrived Tuesday morning—to a range 7 miles below (south) in the same cañon, I did not find any diseased sheep, neither did I find any that had recently died; consequently I had no subject for a *post-mortem* examination. When later in the afternoon the flock was inspected, no animal could be found that showed any symptoms of disease. Once there and convinced by what I learned on making inquiries from Mr. McLawrence and his shepherd that the sheep had died of anthrax, I endeavored to ascertain the cause. I therefore concluded to inspect and to examine the fatal pasture-ground, and as it was only 2 miles above (north), I induced Mr. McLawrence to take me out there. Commencing about $1\frac{1}{2}$ miles from the house, we found the ground strewn with the decomposed and decomposing carcasses of dead sheep, half eaten by the buzzards, and lying in heaps of twos, threes, and fours. Near a water-hole, the largest of the two stagnant pools, about 50 yards long, 10 yards wide at its widest point, and rather shallow, the carcasses became very numerous. I counted about 60 dead sheep in the immediate vicinity of the stagnant pool, and found one in the water itself. Being always provided with some small vials when on such an expedition, I filled two of them with some of the stagnant water and took them with me for microscopical examination. It was very foul and muddy, and its nauseating smell nearly turned my stomach when I filled the vials. When the water was examined under the microscope, on June 23, it was found to contain immense numbers of *spirilla*, *micrococci*, and *bacilli*, the latter undoubtedly *Bacillus anthracis*. It also contained large quantities of granular matter, a good many vegetable rests, some animalcules, and numerous spores of algæ, &c. Consequently no doubt can exist that the water-hole, which, for about two weeks, or from the time the sheep commenced to die till the flock was removed to another range, furnished the drinking-water for some of the sheep, probably, though, only for those that died, as other good water, in the Rio Frio, was equally accessible, and only a mile distant, constituted the source of disease and death. The other stagnant pool, reported to be much smaller and nearly dry, was not visited.

On further inquiry I learned that in the fall of 1881 the same range-

the vicinity of the water hole—had been occupied a few weeks by a herd of cattle, and that about 60 of them had died within a short time. The dying, I was informed, ceased immediately after the cattle were taken away to another place. Further, that in the spring of the same year (1881) a flock of sheep had been grazing a short time on the same range, and that 120 head of them had died in a few weeks, and that they likewise had stopped dying when removed to another locality. Of course none of the dead animals were buried; they all, as is customary in Texas, were left on the ground to rot, and to be devoured by the buzzards and coyotes. The whole range, therefore, is thoroughly infected, and as the land slopes in nearly every direction toward the water-hole which furnished the drinking-water, or at least a part of it, for most of the animals grazing in its vicinity, there can be no doubt that the cause of the fatal disease (*anthrax*), was local, and was caused by drinking the stagnant water contained in the water-hole. That the water was thoroughly infected was proved by the presence of numerous *Bacilli anthracis*. Further comment will hardly be necessary.

Arrived again in Uvalde on the night of June 20–21. Knowing that cyst-worms are of frequent occurrence in sheep in the neighborhood of Uvalde, I procured in the morning, through the kindness of Mr. Abrams, four jack-rabbits, and found every one of them infected with numerous cyst-worms, containing fully developed scolices. The cyst-worms were situated nearly everywhere in the abdominal and pelvic cavities, particularly near the kidneys and urinary organs, but also in and between the muscles. One of the rabbits contained about thirty, and another one over twenty.

In the early part of July (July 3 and 4), on my trip to Corpus Christi, I stopped in San Diego, Duval County, and from there visited several sheep-ranches on which the sheep were reported to be affected with *lombriz*. I had an opportunity of making a few *post-mortem* examinations on the ranch of Knight Brothers, 14 miles from San Diego (July 4), but found nothing of special interest except some *Strongylus contortus* in the fourth stomach, and in one lamb a few *Strongylus filaria* in the bronchial tubes. In 1880 Knight Brothers had a flock of 3,500 sheep, and suffered no losses from *lombriz*; the sheep, the whole year, were in a very good condition, and did not emaciate during the winter. In 1881 they had about 4,300 sheep; the winter was a hard one, and the animals, in consequence, were rather poor in the spring. The losses from *lombriz* in the summer of 1881 amounted to about 400 lambs. In 1882 their flock of sheep numbered 5,000 head, which, owing to a mild winter, were all in a good condition in the spring, at any rate in a much better condition than in the spring of 1881. The losses till date, July 4, have been very few. The range, I was informed, has been in use five years, and no losses from *lombriz* (fatal cases of worm disease) worth mentioning have occurred, except in the summer of 1881. Still, I saw a few lambs evidently affected, but none of them were as yet very

sick. The range, on the whole, is very dry, and almost destitute of water except in a wet season. The sheep, and also a flock of goats, obtain water for drinking from a so-called tank, an artificial pond, which is 400 yards long, and contained on July 4 about 3 feet of water. Knight Bros. are of the opinion that the greater loss in 1881 was caused by the poor condition of their sheep in the spring of that year. As it is a well-known fact that parasites, and entozoa particularly, become as a rule dangerous to higher organized animals only when the latter are weakened, diseased, emaciated, or in a declining condition; that, in other words, parasites flourish the more the weaker the organism of their abode animal; therefore, the reason given for the greater loss caused by *lombriz* in 1881 than in 1882 is a very plausible one. As long as most of the flock-masters in Texas do not make suitable provisions for their sheep in the winter, and do not feed and protect them in bad and cold weather, or when a "norther" is blowing, every severe winter, that is, severe for Texas, will not only cause immediate losses, but will also increase the death rate of the sheep in the following spring and summer. As the present winter (1883) is a hard one it is safe to predict that the death rate next spring and next summer will be a high one, and not only as to sheep but also as to other animals. Although Texas is a southern country and has a warm climate, the changes of weather and temperature are just as severe and just as sudden as they are further north.

In the early part of August I received information that quite a number of sheep were dying near Gonzales, Gonzales County, particularly in a flock owned by Messrs. Stieren and Forke. I went there at once, and together with Mr. Stieren, the senior member of the firm, visited (August 15) their sheep ranch, several miles from the city. The flock consisted of about 3,000 head, including about 1,000 lambs. Of the latter, about 250, or 25 per cent., had died within the last four or five weeks of a disease alleged to be *lombriz*. When we arrived at the ranch we found that the flock had recently been moved by the shepherd to another range, a few miles further north, and that the dying had almost entirely ceased since the removal had been made. Only a few deaths had occurred during the last two or three days. I left it to Mr. Stieren and his shepherd to pick out the sickest lamb in the flock to be killed for *post-mortem* examination. It was killed by bleeding. Examining it, I found grubs (larvæ of the gad-fly) in the sinuses and ethmoid bones, a few large specimens of *Strongylus filaria* in the lungs, some *lombriz* worms—*Strongylus contortus*—in the fourth stomach, and degenerated mesenteric glands. The range, but particularly the old one (the one occupied by the flock till within a few days of my arrival), contains numerous small water-holes, or rather creek-ponds, which become dry in the summer, or in every dry season contain more or less water after a good rain, and form a continuous stream (a creek) only after heavy rains, or in a very wet season. Besides, the old range,

partly at least, is composed of so-called hog-wallow land, and in a wet season each hog-wallow forms a small water-hole. From the above it will be seen that the range in question is not at all adapted to sheep-raising, but admirably qualified to preserve worm-brood.

On August 29 I had an opportunity to examine a diseased flock of sheep belonging to Mr. Boaz, in Fort Worth, Tarrant County. Mr. Boaz bought in June a flock of 271 sheep, among them 25 muttons (wethers), the rest ewes and lambs. These sheep, on the whole rather inharmonious cross-products of Cotswolds, Lincolnshires, Leicestershires, Mexicans, and Merinos, were imported from Missouri in the latter part of March, and since that time have been kept and herded in the vicinity of Fort Worth. In the latter part of June, or immediately after Mr. Boaz took possession, the sheep commenced to die at the rate of one a day on an average. In all 59 animals, mostly lambs, and including only a few full-grown, probably very old sheep, had died when I arrived. The symptoms observed in the animals found diseased were as follows: At first a tendency to remain behind in the herd; a slight drooping of head and ears, then gradually increasing weakness and emaciation; paleness of skin and visible mucous membranes; more or less coughing and sneezing and discharges from the nose (in most of the animals); irregular appetite and digestion, and at times costiveness and more or less fever, till finally the weakness, emaciation, anaemia, and fever increased, and the costiveness gave way to a more or less fetid diarrhea, which, in most cases, was soon followed by death. Mr. Joe Dickson, the overseer of Mr. Boaz's ranch—Mr. Boaz is a banker, and lives in Fort Worth—and myself singled out and caught two lambs, which, in a high degree, exhibited all the symptoms mentioned. The temperature of the animal, taken in the rectum, was $104^{\circ}.8$ and $103^{\circ}.4$ F., respectively. Hence, the first one, having the highest temperature, and being apparently the sickest, was killed by bleeding for the purpose of making a *post-mortem* examination. The morbid changes presented were as follows: *Æstrus* larvæ, a few, in the ethmoid bones; some lung-worms, *Strongylus filaria*, in the bronchial tubes, but unmistakable indications that many more had been present, which had probably been ejected by coughing and sneezing, and an immense number of *lombriz* worms, *Strongylus contortus*, in the fourth stomach. Besides that, the mesenteric glands presented considerable fatty degeneration. No other morbid changes could be found. The worms in the fourth stomach were so numerous as to give the whole mucus lining of that organ an appearance somewhat similar to that of a coarse felt-saddle-blanket, in which every hair is curly, in motion, and wriggling. Consequently there can be no doubt that I had before me a case of veritable *lombriz*, one that would have terminated in death if the animal had not been killed by bleeding. As Mr. Boaz kept on the same range another flock of sheep, animals of his own raising, and crosses between

the common Mexican sheep and American Merinos, which were all, without exception, in a good and healthy condition, it became evident that the imported sheep did not become affected while on his range or in his possession, and that they must have picked up the worm-brood at some other place. Still, even his range, or at least a portion of it, contains numerous hog-wallows, and low, wet places, and therefore all the elements necessary to preserve the worm-brood, if once deposited. Such is the case on a great many Texas sheep-ranges. Of course, as long as no worm-brood is present or introduced, hog-wallows, low, wet places, water-holes, small ponds, &c., cannot produce it, but if worm-brood is once introduced, those places afford all the conditions necessary to the embryonic life of the worms belonging to the genus *Strongylus*, and of a good many others. The worms usually are introduced with imported sheep, which deposit the worm-brood on the grass, or directly in the water, and if then the conditions necessary to the embryonic existence of the entozoa are given, a range once infected, very likely will remain infected till abandoned, for at least a year or two, as a sheep-range. I therefore advised Mr. Boaz to keep his sheep away from the hog-wallow portion of his range, and to use the same, at least a year or two, exclusively for cattle, so as to give the worm-brood a chance to die out.

On August 30 I went again to Mr. Boaz's sheep-camp, and killed another lamb for *post-mortem* examination. Its temperature before death was 104°·4 F. The carcass was found to be very anæmic; head and lungs were perfectly healthy, but innumerable "*lombriz*" worms, *Strongylus contortus*, were found in the fourth stomach. No other morbid changes were present, except some degeneration of the mesenteric glands, and a few rests of aborted cyst-worms in the mesenterium. The result of this second *post-mortem* examination convinced me still more that in Mr. Boaz's herd I had to deal with severe and fatal cases of *lombriz*. Hence, finding in the imported flock quite a large number of animals, both lambs and sheep, in a condition very similar or even identical to that of the two lambs killed for *post-mortem* examination, I considered the herd a very suitable one to experiment with as to medical treatment, &c. As the worms are in the stomach, and therefore exposed to the effect of the medicines given, the only difficulty consisted in selecting from among the large number of anthelmintics known to *materia medica* one that is effective, requires as little preparation as possible, is easily administered to a large number of animals, and is not too expensive. The Texas flock-master has, as a rule, very large flocks, and concerning a medical treatment will only consent to give such remedies as are easy and simple of application, and can be given in a kind of wholesale manner. I therefore chose a solution of tartar emetic in rain-water, in a proportion of ten grains to two ounces, or half a pound of tartar emetic to twelve quarts of rain-water, and desired to give to each lamb, according

to its size and age, from one to two ounces of that solution as a drench, and to older sheep a little more, in proportion to their size and weight. The result was an excellent one; all those animals not already too weak or too anæmic to be past recovery, even if free from worms, recovered. The tartar emetic not only freed the animals of *Strongylus contortus*, the "lombriz" worm, but also a large tape-worm, *Tenia expansa*, from a lamb that was supposed to be suffering only from lombriz. Besides that, sheep will stand without damage a comparatively large dose of tartar emetic, much larger in proportion than any other domesticated animal. As it is essential in administering anthelmintics to animals that the stomach of the latter at the time the medicine is given be not too full of food, the best way to give the solution of tartar emetic to a large flock of sheep, is probably as follows: First, drive the whole flock, or as many sheep as can be drenched in one day or an evening, into the shearing-pen, which is on most sheep-ranches the best suited place for the purpose, and leave the sheep there over-night without any food. Then in the morning employ three men and a boy to do the necessary work. One of the men has to catch the sheep, and to lift them, one by one, as they are wanted, over a fence, when another man takes hold of them, and holds each sheep while it is drenched. A third man, who may be the owner or overseer of the flock, attends to the drenching, and gives each sheep or lamb its proper dose out of a small bottle—a two-ounce vial I would prefer—and in small swallows, for then the medicine will almost immediately reach the place where it is wanted, the fourth stomach, whereas, when given in big swallows, or poured down in a hurried manner, it is apt to pass into the paunch, and will then, mixed with the contents of the latter, lose much of its efficacy. The boy, or the fourth person, must have his place near the man who does the drenching, and attend to the filling of vials in use. The solution itself may be kept in a bucket handy to the boy. If proceeded with in that way, 500 head of sheep can easily be drenched in one day. In order to prevent the sheep from filling their stomachs with food immediately after they have taken the medicine, it may be well to keep them an hour or two in the inclosure after the last one has been drenched. There are, of course, other effective remedies besides tartar emetic. Some flock-masters claim to have seen good results from large doses of common salt, while others have used oil of turpentine with good success. As to oil of turpentine nothing can be said against it, except that with safety it can only be given in the form of an emulsion, and to make an emulsion for a large flock of sheep is much more trouble than the average Texas sheep-man is willing to take. The late Professor Gerlach, in Berlin, recommended creosote in the water for drinking, and considered two ounces and a half a sufficient dose for 100 sheep. As I was not able to obtain a reliable article when I had use for it, I did not test its efficacy, but hope that others will. There will be abundant opportunities next summer.

CONCLUSIONS.

As will have been seen from the above, so-called *lombriz* becomes a destructive disease or causes severe losses only if the affected flock of sheep is in a bad—weak, emaciated, and anæmic—condition, or where the range is badly infected with worm-brood and not adapted to sheep-raising on account of the favorable conditions it offers for the preservation of entozoa (worms) while in their embryonic state. I would exclude from a sheep-range, but particularly from a lamb-range, first, all such land as is low and level and inclined to be wet; secondly, all so-called hog-wallow or buffalo-wallow land; thirdly, all such land as contains small pools of stagnant water, not to speak of swamps, sloughs, and swales. It is true sheep need but little water compared with what is needed by other animals, but they need some, and what they need is water of the very best quality, or water that is as free as possible from decaying organic substances, and consequently free from (pathogenic) schizophytes and worm-brood. Really good, pure water, good well-water in particular, is not apt to contain dangerous schizophytes, neither is it well qualified to preserve and to sustain the life of embryonic worms, consequently is not productive of disease. As most of the sheep-ranges in Texas are rather scantily supplied with good water, the flock-masters who own their range, as most of them do, and have not a sufficient supply of good water, will be obliged to dig wells—artesian wells where necessary—and doing that they will soon find that the money thus invested will bear big interest.

I made one visit to a sheep-ranch in Hays County at which the sheep were dying, and found the cause to consist exclusively in the bad and filthy condition of the stagnant water of the rapidly-drying pools out of which the sheep had been drinking. There is still one other thing that deserves the special attention of the Texas flock-master. Every year a great many improved sheep are imported into Texas, principally from California, Vermont, Ohio, Michigan, and from several other States, and these imported sheep are the ones that introduce not only scab and foot-rot—which latter, by the way, is a very rare disease in Texas—but also every kind of worm-brood. The Texas flock-masters, therefore, in importing sheep or in buying imported sheep, cannot be too cautious in seeing to it that he gets perfectly healthy animals, belonging to healthy flocks, and none that are suffering from or infected with any of those diseases named. Most of the diseased sheep or sheep afflicted with entozoa that are imported are brought in by dealers, and by them are sold usually in lots, which contain good ones and poor ones, to the highest bidder or in open market, after they have been driven through the whole State or a large portion of the same, and thus been permitted to infect more than one range. Besides that, they usually come in the fall, the very season in which most of the worm-brood is disseminated. What remedies or preventives may be necessary to avoid an infection

of a sheep-range with worm-brood may be concluded from the above. It is not for me to say what ought to be done. It is for the flock-masters themselves to act and to decide, or to adopt such measures as will meet the object in view.

Very respectfully submitted.

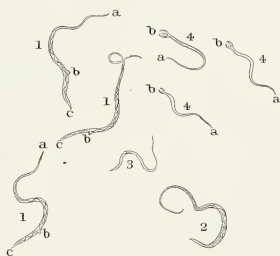
H. J. DETMERS.

CHAMPAIGN, ILL., *January 29, 1883.*

DISEASES AMONG SHEEP IN TEXAS.

Investigations of D^r H. J. Detmers.

Plate I.



Group of *Strongylus contortus*, natural size.

1,1,1, full-grown females

2, not fully developed female

3, young female

4,4,4, full-grown males

a,a,a,a, head,

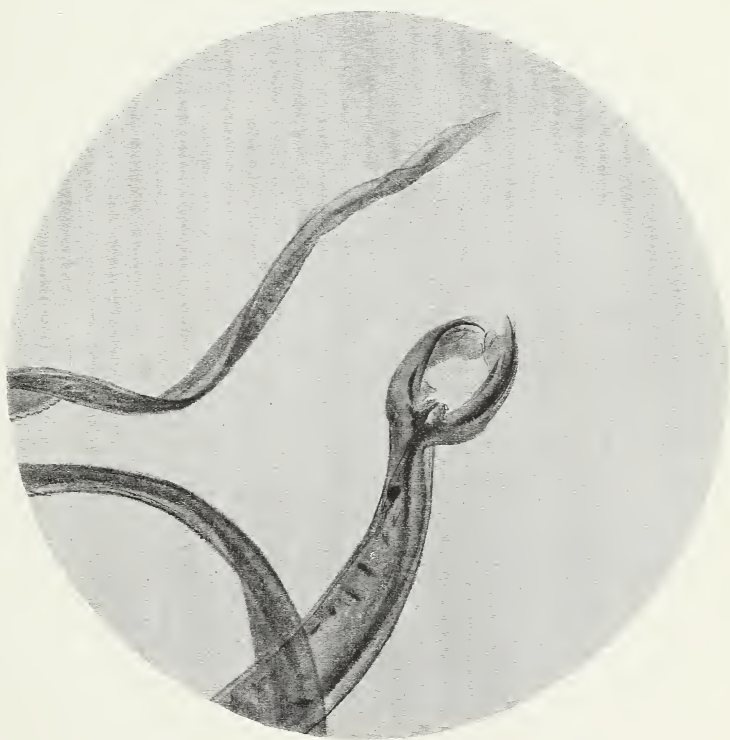
b,b,b,b, genital organs of both sexes.

c,c,c, tail and anus of female.

DISEASES AMONG SHEEP IN TEXAS.

Investigations of D^r H. J. Detmers.

Plate II.

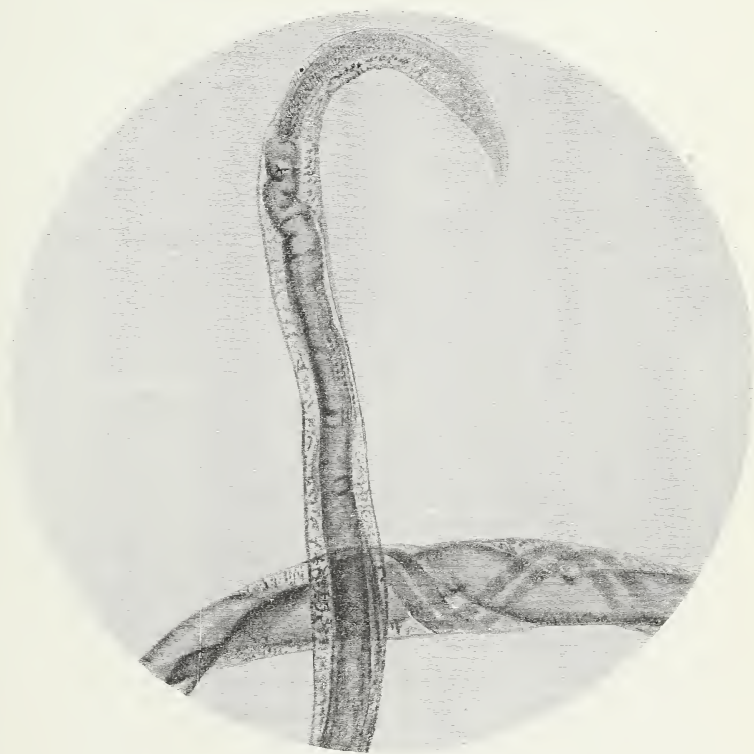


Microphotograph of head and tail of male *Strongylus Contortus*—x 40.

DISEASES AMONG SHEEP IN TEXAS.

Investigations of D^r H. J. Detmers.

Plate III.

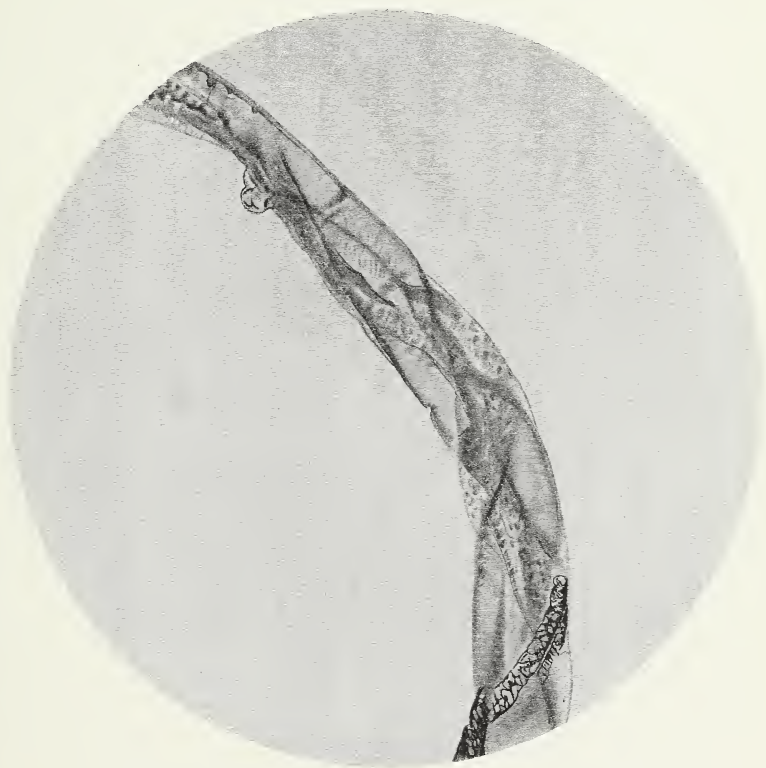


Microphotograph of head of female *Strongylus Contortus*—x 40.

DISEASES AMONG SHEEP IN TEXAS.

Investigations of D^r H. J. Detmer's.

Plate IV.



Microphotograph of middle portion of female *Strongylus Contortus*—x 40.

DISEASES AMONG SHEEP IN TEXAS.

Investigations of D^r H. J. Detmers.

Plate V.



Microphotograph of tail of female *Strongylus Contortus*—x 40.

FOOT-AND-MOUTH DISEASE IN GREAT BRITIAN.

By EDMUND J. MOFFAT, *London Agent U. S. Department of Agriculture.*

LONDON AGENCY, *February 27, 1883.*

For months past isolated cases of foot-and-mouth disease have been reported from time to time without attracting serious attention on the part of the authorities, but lately the rapid spread of the disease in every part of the kingdom has naturally caused great alarm, and energetic measures have been set on foot to stamp out the infection.

The importation of cattle from Scotland into Ireland has been prohibited. Orders of council have been issued allowing sales of fat stock to be held upon license being granted by the local authorities in which the market is situated, but all animals exposed at the sale are to be marked, whether sold or not. The animals must not afterwards be exposed at any public sale, but must be slaughtered within six days. The order further provides that a public or private sale of animals, fat or store, may be held without a license of the local authority in any case where the sale is held on a farm or premises not in a place infected with foot-and-mouth disease, and when the animals exposed at the sale have been on the farm or premises not less than fourteen clear days immediately before the day on which the sale is held. The last order issued, on the 23d of February, arms the local authorities with power to prohibit or regulate the importation of animals into their respective districts from the districts of any local authority in the United Kingdom.

On the 19th of February the House of Commons had a general discussion on the subject of the depression in agriculture, and the pressing topic of the prevalence of disease among the flocks and herds naturally came up for debate. The Right Honorable A. J. Mundella, vice-president of the council, was called upon for a statement of the present condition of the country in regard to cattle disease, and in the course of his speech made some important remarks touching the intentions of the Privy Council in the premises. Among other things, he said that the steamship *Kansas*, from Boston, U. S. A., had arrived at Liverpool on 30th of January having on board 313 cattle and 398 sheep. The cattle inspector stationed at Liverpool certified on the same day that 219 of the cattle were affected with foot-and-mouth disease and 6 sheep with sheep-scab. The cargo was landed at Birkenhead and there slaughtered.

Referring to the future action of the Government in regard to the matter, he said that—

It is not the intention of the Privy Council to prohibit importation from foreign countries where disease is known to exist. He was advised that there is no country in the world where disease as defined by the contagious-disease (animals) act of 1878 is non-existent, and that prohibition would be in direct violation of the principle of the act, which distinctly provides for the reception and slaughter of animals coming from countries where disease is known to exist. At present all imported cattle were slaughtered except those coming from Scandinavia.

Colonel Harcourt said that it was time for the Government to entirely prohibit the importation of foreign live stock, as slaughtering at the port of embarkation did not prevent the disease spreading. In this suggestion he was seconded by every member who took part in the debate.

In answer to the criticisms of the members on the attitude of the Government, Mr. Mundella said that the importation of live animals was larger in 1882 than for any year in the last fifteen years. The price of meat had largely increased in this country; the price of mutton was unquestionably high, and beef was higher than it had been for many years. Now, the honorable gentleman declared that it was the duty of the Government to entirely prohibit, without unnecessary delay, the importation of live foreign cattle. But what would the effect of that be on the supply of meat and the prices of meat in this country? In 1881 the number of live foreign animals imported was 1,471,891, of the value of £8,526,000, and in 1882 the number imported was 1,483,761, and the value £9,722,000. Now, if they abstracted this supply from the country the prices of meat would be enormously affected; in the opinion of an authority competent to judge, would make a difference of 2*d.* or 3*d.* a pound. The argument of the honorable gentleman, that if this supply was cut off it would be met with a greater supply of dead meat, was not borne out by experience, because the cutting off of the supplies from Germany and Belgium, through fear of cattle plague, did not lead to the importation of a pound more of dead meat. The dead-meat trade, so far from increasing, had steadily diminished. In 1881 812,000 cwt. of dead meat was imported, while in 1882 the quantity was only 460,000 cwt., the value falling from £2,163,000 to £1,282,000. He had always understood that one of the greatest boons the Government ever conferred upon the agricultural community was by passing the contagious diseases (animals) act, 1878, and yet the honorable member seemed to regard the measure as of no value whatever. With respect to the recommendation of the royal commission, he contended that the noble president of the council voted against the proposal of the honorable member, and that some of the most experienced members of the commission voted against the paragraph which he carried in that commission. The honorable gentleman had given a history of imported disease since the accession of the Government to office. On that point he would state exactly what had happened. Since the act of 1878 was passed

there had been no cattle plague and no sheep-pox, while the ravages of pleuro-pneumonia, the disease against which the act was mainly directed, had steadily decreased. In 1877 there were 2,007 outbreaks of pleuro-pneumonia, and 5,000 animals were affected; in 1882, after an interval of continual decrease, there were only 491 outbreaks and 1,200 animals affected. It was to be hoped that in time the disease would be wholly stamped out; indeed, he might venture to say that the authorities were now in a fair way to do so. How did the case stand with regard to foot-and-mouth disease? The honorable gentleman had said that from January to October, 1880, the country was free from that disease, but that soon afterwards three diseased cargoes arrived and reintroduced it. That was not a perfectly accurate statement, for during the whole of that time diseased cargoes were steadily coming in. As many as thirteen, he believed, reached our shores in the course of those nine months. Still, during that period the country was free from disease. The year 1880 was, no doubt, exceptional, for in the whole of the 12 months there were only 1,461 outbreaks and 32,000 animals affected. In 1881 there were 4,833 outbreaks and 183,000 animals affected. In 1882, which was almost as good a year as 1880, there were only 1,970 outbreaks and 37,000 animals affected—37,000, it should be remembered, out of a total of more than 33,000,000 animals in the country. That was the disease which according to the honorable gentleman, had played such havoc among our stock, and yet a much worse, and certainly not imported, disease was the fluke among sheep, which was directly caused by wet weather, and had carried off nearly 3,000,000 sheep in one year. Now, was it fair, in the face of these facts, to speak of imported disease? The honorable gentleman knew that no one could possibly do more than the Privy Council had done to arrest the progress of the disease, and freely admitted that they had worked with considerable success; and yet he called upon the Government to prohibit the importation of foreign cattle.

In 1878 the honorable gentleman and those who supported his views declared that no idea of any such prohibition had ever entered their heads. The act was passed, and the schedule relating to foreign cattle met with the approval of the honorable gentleman and his friends. The act had been strictly enforced, and every country, where the disease had appeared, had been duly placed in the schedule. The honorable gentleman said at the time that all he wanted was that cattle from infected countries should be slaughtered at the port of debarkation, and that was precisely what had been done and was being done at this moment. His honorable friend behind him had spoken of the severe restrictions imposed on the importation of cattle, and had wondered that the Government did not recoil from them. All he could say was, that it was his duty to administer the act as he found it, and that he had endeavored to do so to the utmost of his power. As for restrictions imposed by the closing of markets, he should like to ask his honorable friend, the mem-

ber for South Leicestershire, whether he thought the Government had done too much in that direction. The result had certainly been satisfactory. Until the last outbreak Norfolk had been free from the disease, and the order issued on December 11 had reduced the number of outbreaks in Norfolk from 45 to 35 in one week, in the next week from 35 to 11, then to 9, and finally to *nil*. Much the same thing had happened in Northamptonshire and Suffolk, though he was not now as sanguine as he had been of complete success. At any rate, the Government would do its best to stamp out the disease. As this question would come on again when the honorable member for Lincoln brought on his motion, he would reserve his remarks until that time. As regards the alleged falling off of cattle, he would observe that so far from falling off the numbers had risen from 4,900,000 to about 6,000,000, and there has been an increase even since last year, and that notwithstanding the high prices of stock. In 1867 there were 4,900,000 cattle in Great Britain, and in 1873, 5,900,000, and the highest number had touched about 6,000,000, so it was incorrect to say there had been a falling off in cattle. With the present terrible disease in sheep it was to be wondered at that the numbers were maintained as high as they were. Both the present and the late Government were agreed upon one point, that the importation of cattle ought not to be stopped. Foreign countries might send as many cattle as they pleased, but they must be slaughtered at the port of debarkation. Lord Sandon himself pointed out that honorable members were mistaken in supposing that the bill stopped importation. What the Government said was, that the animals might be landed alive, but they must be slaughtered at the port of debarkation. If they were slaughtered at the port of embarkation, as it was suggested, the result would be famine prices in meat, and thus much misery would be caused in this country.

There appears to be great dissatisfaction with the views of Mr. Mundella as expressing the intentions of the Government, and the total prohibition of the importation of all foreign live stock is energetically demanded, and this demand is not likely to grow weaker if the feeling on the subject is accentuated by a repetition of the case of the steamship *Kansas* bringing diseased cattle from the United States. The lesson cannot too often or too strongly be urged upon the meat exporters of the United States that if they desire to perpetuate and increase their trade with Europe the utmost care must be exercised in the selection of the best qualities for this market, and that the exporting of diseased cattle is a crime which will react in such a way as to totally ruin the trade. The indirect results of such a course are far more serious than the direct, for the mere loss of a shipment of cattle is nothing compared with the impression that is spread throughout Europe that only the surplus and the second qualities of cattle that we do not need are exported. A few dishonest dealers, representing the smallest fraction of the trade, can by their practices close every port in

Europe against our live cattle and sheep, as was temporarily done two years ago against our pigs, and with the prevalent sensitive feeling in England and on the Continent on the subject of cattle disease every precaution should be taken in shipping cattle to leave no ground for complaint.

LONDON AGENCY, *March 17, 1883.*

On the 10th of March the returns from the infected districts show that the disease, although yet very serious, appears not to be spreading as rapidly as formerly, and hopes are expressed that the latter returns will reveal an improvement in the situation. For the week ending February 17, 879 farms and other places were reported infected; February 24, 1,062, an increase of 183; March 3, 1,152, an increase of 90. Of the 1,152 infected places for the week ending March 3, 197 were in Scotland. Of the English counties, Lancaster had 237; York, 154; Stafford, 54; Chester, 49; Derby, 35; Kent, 32; Leicester, 29; Essex, 28; Salop, 24; and the Metropolitan district, 47. Norfolk showed almost a clean bill of health, only two cases being reported. Wales had 39, of which 22 were in Anglesey. Perth heads the list in the Scotch counties with 35 infected places; Edinburgh, 24; Forfar, 27; Fife, 25; Lanark, 21. England had 35 counties out of 40 infected; Wales 5 out of 12, and Scotland 15 out of 33, showing the insidious and contagious nature of the disease, which has spread from one end of the kingdom to the other.

At a meeting of the Royal Agricultural Society, Colonel Kingscote presented the following report, drawn up by Professor Robertson, respecting foot-and-mouth experiments at the Royal Veterinary College:

Since the last report a fresh trial has been made in the same direction as the previous experiments, and on the same lines, viz., testing the susceptibility of home-bred animals to be contaminated directly from material yielded by the imported. This time the experiment has been attended with results clear and positive. In the beginning of December, 1882, having obtained two yearling steers, these were placed in clean and previously prepared boxes, and daily observations made as to their general health and internal temperature. On the 12th December, a quantity of saliva and other secretions of the mouth, obtained from cattle slaughtered at Deptford while suffering from the fever, was obtained, and at 5 p. m. a portion of this was dropped on the muzzles of both steers, their state of health being at that time apparently good—the temperature of No. 1, 103° F.; of No. 2, 102° F. Twenty-four hours afterwards both animals were feeding well, although exhibiting a trifling rise in temperature. On the morning of the 14th, No. 1 was dull, not disposed to rise, and taking little food, the temperature 105°·4 F. No. 2 seemed in every respect natural. Towards evening No. 1 showed a little frothy saliva at the angles of the mouth, with an occasional cough, but no appearance of vesicles on the buccal membrane. The morning following, this animal gave unmistakable evidence of being very ill; it was not inclined to rise, refused food; the temperature was now at its height, 106°·3 F.; while over the tongue, dental pad, and buccal membrane were several well-developed vesicles. These latter did not show between the digits or around the coronet until the day following, the 16th. At this time several of those in the mouth had ruptured. From the 15th to the 19th this animal's temperature continued high, settling on the latter date at 101° F. From the period of the rupture of the first vesicles until the 21st, a wash of 5 per cent. of salicylic acid in a solution of acetate of am-

monia, which was afterwards diluted with an equal quantity of water, was employed for the local lesions. This seemed to expedite their healing; not, however, I believe, more than some other mild astringents do. On the morning of the 17th, finding that No. 2 steer showed no evidence of being affected from the employment of the saliva on the 12th, a little of the same material from his fellow, now in the height of the disease, was taken, and gently smeared over his muzzle. No indication of disturbance occurred until the morning of the 22d, when, besides being somewhat dull and disinclined to eat, the internal temperature rose to 103° F. Vesication on mouth and feet appeared next day. Although this animal seemed to suffer more from the local lesions than his fellow, the temperature never reached the same height, 104°·6 F. being its maximum. On the 29th of December a yearling steer which had been reared at the college, but had not been in contact with the affected animals, except that he was attended by the man who waited upon these—not, however, being fed or watered from the same vessels—showed symptoms of the fever; in a few days he passed through the different phases of the disease in a similar manner to the inoculated. On the 28th of January, when the animals had fairly recovered from the induced fever, a fresh supply of saliva was procured from the same source as the first, and sprinkled over the muzzles of both animals, as before; also, and at the same time, sixty minims of the liquid were injected into the subcutaneous tissue in the inferior part of the neck of No. 2. Succeeding this treatment, no indications of general disturbance followed, with the exception of a slight elevation of temperature. On the 30th, a circumscribed swelling of 2 or 3 inches in diameter, and very tender, appeared around the puncture which had been made in the skin of the neck; this in a few days began to discharge a little disagreeably smelling fluid, and continued to do so for a week, shortly after which time the local infiltration had entirely disappeared. These two animals have been sold, and two others purchased. With these latter it is now intended to try the effects of the direct conveyance into the blood stream of the disease-bearing fluid of the vesicles, with the object of determining whether a modified fever may be produced which shall confer a protection from the natural fever.

LONDON AGENCY, *April 20, 1883.*

The report for 1882 of the Veterinary Department, under the direction of Prof. Brown, has just been issued, and presents, as usual, a full consideration of all questions relating to cattle diseases. Professor Brown devotes several pages to the discussion of the three propositions advocated in Great Britain to prevent the introduction of disease, *i. e.*, prohibition of landing of animals from countries where foot-and-mouth disease exists; prohibition from all countries which are not free from all cattle diseases; and the more sweeping proposition to exclude animals from all countries permanently, and substitute a dead-meat for a live-meat trade. The first he considers rather a means of prohibiting the continued importation of animals affected with foot-and-mouth disease, than as an effectual method of totally excluding it from the country; the second is looked upon as an "incomplete measure" very difficult to carry into effect, not calculated to afford anything like perfect security against the introduction of foreign diseases, and not at all likely to lead to a permanent and radical change in the meat trade. After discussing the disadvantages of the third measure, he adds:

Under all the circumstances it would be well to be prepared to accept all the consequences which would result from a withdrawal of the whole live-meat supply we receive from the continent of Europe. It is recognized as a fact that Europe is no longer able to supply meat enough to feed her population, and perhaps the strongest

argument in favor of the substitution of a dead-meat for a live-meat trade may be based on the assumption that sooner or later we must supplement our deficient food supplies by drawing upon the vast resources of the Colonies.

In this conclusion as to absolute prohibition he is in accord with the general agricultural sentiment of the country, as opposed to the stand which has been taken by the Government on two occasions lately.

As to the sanitary condition of native stock, the report states that no serious outbreak of disease is to be noted, and that pleuro-pneumonia has almost ceased to exist over a large portion of the country, the total number of outbreaks only amounting to 494 in 1882, against 729 in 1881, while the annual average for the five previous years was 1,701. In this connection it is greatly doubted whether the outbreaks for 1882 were as many as reported by the inspectors, as in 247 outbreaks of the 494 of the year the disease ceased after one or two animals had succumbed, which is regarded as an exceptional event in the history of the disease. The total number of outbreaks of foot-and-mouth disease was 1,970 in 1882, against 4,833 in 1881. The number of animals attacked in 1882 was 37,950, against 183,046 in 1881. In comparison with the number of live stock in the country these figures are insignificant, but in order to keep the disease within the limits to which it was confined it was necessary to impose restrictions which in some cases were extremely onerous.

Foot-and-mouth disease is as infectious as cattle plague, and its extirpation demands equally stringent—or if slaughter is to be omitted, even more stringent—regulations than those which have been found necessary for the eradication of the more fatal malady.

Swine fever has increased. The outbreaks in 1882 were 2,983, against 1,717 in 1881, and the animals attacked reached the total of 14,763 in 1882, against 7,994 in 1881. The increase is attributed to the highly infectious character of the disease, and the lax manner in which in many districts it has been treated. The obscurity of the indications of the disease is such that many healthy animals are often contaminated before the presence of the disease is known. It is feared that swine fever will never be eradicated until the slaughtering of all animals which have been exposed to infection is universally adopted. No cases of cattle plague or sheep-pox were reported during the year. Sheep-scab shows an increase, there being 2,234 outbreaks and 38,509 sheep attacked in 1882, as compared with 2,055 outbreaks and 32,571 sheep attacked in 1881. Glanders has decreased from 858 in 1881 to 531 in 1882. Very few of the cases have proved fatal. Farcy was reported in 531 cases. Glanders and farcy are mostly confined to London and vicinity.

DISEASES AMONG FOREIGN ANIMALS LANDED IN GREAT BRITAIN.

Only five foreign cattle affected with pleuro-pneumonia were landed, three of which came from the United States and one from Portugal. The returns allege that in 1880 there were 229 cattle affected with this

disease brought from the United States, against 39 in 1881. Foot-and-mouth disease was more serious in imported cattle, sixty-six cargoes having diseased cattle on board being landed from the Continent during the year. No cases are reported from the United States. The ports of Boulogne in France, Corona in Spain, and Oporto in Portugal, were temporarily prohibited from forwarding cattle by order of the Privy Council.

A statement is given of the condition of cattle as respects disease in different countries during 1882. Austria: Cattle plague appeared in several provinces, but has declined. Anthrax was very prevalent in Hungary. In Belgium legislation for the extinction of pleuro-pneumonia has not been successful. Denmark: Condition of animals healthy; the prohibition of the importation of cattle from Sweden into Denmark has been removed. Cattle and sheep from Denmark are not subject to slaughter at the port of landing in England. France: Foot-and-mouth disease has again been extensively prevalent throughout the country. All animals coming from France to England must be slaughtered. No outbreak of cattle plague has been reported in the Empire of Germany during the year 1882, but foot-and-mouth disease has been prevalent in various parts of the country. No outbreaks of sheep-pox are reported. There were several cases of splenic fever in cattle. Exportation of cattle from Germany (except Schleswig-Holstein) to England is still prohibited. The sanitary condition of animals in the Netherlands was very good during 1882, and the constant effort to stamp out pleuro-pneumonia has been successful, the disease now being entirely confined to the Spoeling district. Exportation of cattle and sheep from the Netherlands to England is increasing. The number of cases of pleuro-pneumonia has decreased from 6,079 in 1881 to 62 in 1882. Italy: Foot-and-mouth disease has been exceedingly prevalent all over Italy during the year, and outbreaks have occurred in the Island of Sardinia. Outbreaks of malignant carbuncle and cases of red water have occurred. Importation of animals from Italy is prohibited. Cattle plague does not appear to be on the decrease in Russia, and importation from that country is prohibited. Foot-and-mouth disease exists both in Spain and Portugal. Notwithstanding the order to slaughter animals coming into England from these countries, the export trade is on the increase. Owing to the superior sanitary condition of animals in Sweden and Norway, imported animals are not subject to slaughter. Few cases of foot-and-mouth disease are reported from Switzerland. In consequence of the prevalence of cattle plague, sheep-pox, carbuncle fever, and splenic fever, the importation of all animals from Turkey is prohibited.

NEW ORDERS OF PROHIBITION.

In England, Scotland, and Wales, on the 1st of January, 1883, the landing of cattle, sheep, and goats, and all ruminating animals, and swine brought from the following countries was prohibited: Austro-Hungary, Greece, Italy, Montenegro, Roumania, Russia, Turkey; the

landing of cattle brought from Germany and Belgium, and cattle, sheep, or goats being, or having been, on board a vessel at the same time with animals so brought. No prohibition is issued against animals coming from Denmark or Sweden; swine coming from these countries, however, are to be slaughtered at the port of landing. All animals brought from Canada, Norway, and the Channel Islands are allowed to be landed without being subject to slaughter or quarantine. Cattle, sheep, goats, and swine brought from any other countries are only allowed to be landed at the foreign animals' wharf for slaughter.

Turning now from the consideration of the year 1882 to the events of the first three months of 1883, which have witnessed the great spread of foot-and-mouth disease in Great Britain, and the introduction of several large cargoes of infected cattle from the United States, a significant change is to be noted in the opinion of all agricultural bodies and authorities in the means to be adopted to stamp out the disease. The third proposition of Professor Brown, *i. e.*, to total and permanent prohibition of all live stock from all countries, and the substitution of a dead-meat for a live-meat traffic, is now strenuously advocated, without a dissenting voice, except that of the Government, which has twice, once in the person of Mr. Mundella, and more lately by Lord Carlingford, checkmated the proposal. On the 16th of April, in the House of Lords, the Duke of Richmond and Gordon, who was the chairman of the late royal agricultural commission, made a very strong appeal to the Government to adopt extreme measures, especially with reference to the United States. Last year our bill of health left nothing to be desired; but the two large cargoes of diseased cattle that have lately arrived have turned public opinion against us, and the demand is that slaughtering in our case has proved insufficient, and that prohibition is the only remedy. Whether the inspectors here have acted mistakenly, as they have done before; whether the American cattle, perfectly healthy on leaving our shores, have been contaminated by the vessel or the head-ropes; whether the cattle have caught the disease after landing makes little difference, as far as the public opinion goes. The main fact that American cattle have been condemned for having disease is sufficient for the purpose; and, as I have pointed out in previous communications, a few more such unfortunate cases will, in all probability, cause such a pressure to be brought upon the Government that American cattle will be placed in the prohibitive list, and not even be allowed the present grace of being slaughtered at the foreign animals' wharf. If extreme measures should be adopted, I do not doubt but that the United States could hold its own with other countries in supplying dead meat; but as long as we have both privileges, the live-meat trade ought not to be jeopardized if human foresight and care can be of any service. What has been done with Russia and Turkey can as easily be done with American cattle, although our active exertions in stamping out disease and our anxiety to show a clean bill of health are well known.

CONTAGIOUS PLEURO-PNEUMONIA.

REPORT OF WILLIAM B. E. MILLER, D. V. S.

HON. GEORGE B. LORING,
Commissioner of Agriculture:

SIR: In accordance with my duty as one of the veterinary staff of your Department, I have the honor to submit the following summary statement of the work done from October 31, 1881, the date of last annual report, to December 31, 1882.

The system of inspection and quarantine established during the previous year has been successfully continued during the present season, and has proven much more satisfactory in its results, as many of the inconveniences and delays that were at first impossible to prevent have been overcome in a measure, and while in a few instances detentions and difficulties have necessarily occurred, yet in the main the work of inspection has been satisfactorily accomplished. I have been very much aided and assisted in my labors by the superintendents and officers of the several ferries and of the railroads centering at this point, who have not only given every facility in their power to render the work of inspection easy and rapid, but who have refused to transport cattle that were unattended with a certificate of examination or permit, thus preventing evil-disposed persons from evading the law of quarantine and examination.

To the superintendents of the stock yards of Philadelphia I am indebted for the invitation to visit their yards whenever I so desired, either for purposes of examination or observation, and for valuable information on several occasions. I have been very much benefited from the assistance rendered by the secretary of the State board of health of New Jersey, Dr. E. M. Hunt, who has had the charge of carrying out the law of said State, relating to the suppression of the contagious diseases of domesticated animals. From information received either directly from him or through his assistants, I have in several instances been enabled to ascertain the exact location where diseases in animals were known to exist, and to learn all the facts in connection with the same without having to make a personal visitation. I have acted in perfect harmony with the board of health, and have promptly reported to the secretary all cases of a contagious or infectious character that have been found within the limits of the State. During the past fourteen months I have examined about 16,000 cattle, of all ages and descriptions, the majority of which were centered at this point. The

balance were located in New Jersey, New York, Pennsylvania, Delaware, Maryland, Virginia, West Virginia, Ohio, and Illinois. Many of them have been afflicted with the ordinary diseases incident to cattle, and quite a number with diseases of an infectious or contagious character. Those coming under my immediate observation, and which have proven the most fatal during the year, have been Texas or splenic fever; phthisis pulmonalis verminalis; (hoose or husk) in young animals; diarrhea in young calves, and puerperal fever in fresh cows. The first mentioned has been located in each of the States above mentioned, and has been seriously fatal. The second has been quite seriously fatal in New Jersey, and I have no doubt is quite as much so in other States wherever it has made its appearance. The third is of daily occurrence at the stock-yards at this point where the traffic in young calves is very extensive, and the fourth has been very prevalent in this locality during the past year.

There has been a marked improvement in the general health and appearance of young calves at the ferry-yards, owing, no doubt, to the system of inspection, which has had the effect of making the dealers more careful in making their selections of stock, knowing that unless they were in good health they would not pass inspection. This fact is also plainly demonstrated in animals of larger growth. Of contagious pleuro-pneumonia I have found in all nineteen cases. Most of them were reported as having originally been purchased at the Baltimore stock-yards. Some, however, were old chronic cases, found at sundry times among cows that were reported as purchased in Burlington, Gloucester, and Salem Counties, New Jersey, and were taken to the slaughter-house for bologna beef, they having sufficiently recovered, had been fattened for that purpose, and when detected were in charge of a dealer and drover of bologna stock from Philadelphia. Two others were found among a herd of cattle that were reported as having come from Delaware County, Pennsylvania, about the 1st of April last, both of which were chronic cases. Recent reports from the commission appointed by the governor of Pennsylvania would seem to warrant the assertion that this disease is now almost entirely eradicated from that State, and personal investigation has demonstrated the fact that there are but very few infected herds in New Jersey, in nearly all of which the source of infection was directly traceable to the stock-yards of Baltimore or New York and Brooklyn. All of these infected herds are in charge of the State board of health and are closely guarded and quarantined. There is therefore but little danger of the spread of the disease from any of them, and was it not for the constant danger of further infection from the above-named sources, the State board of health, in whose charge the matter rests, would very soon be enabled to make a similar report to that so recently made by the agent of the governor of Pennsylvania, namely, that the State was entirely freed from contagious pleuro-pneumonia. In the discharge of my duty as your in-

spector I have visited nearly every county in the State, and am fully satisfied that these statements are justified by the facts in the case.

On the 16th of August I received instructions from your Department to proceed at once to Steubenville, Ohio, and investigate the nature and extent of a disease then existing among cattle in that vicinity. I started forthwith, reaching my destination on the evening of the 17th. I spent several days in that locality and in Brooke County, West Virginia, across the Ohio River, at or in the vicinity of Wellsburg. A special report to your Department, dated August 22, 1882, fully sets forth the results of that investigation. On or about the 23d of August I received information, through Dr. Charles K. Dyer, an assistant inspector of the State board of health of New Jersey, that a disease thought to be splenic fever had made its appearance on the farm of Joel Haines, Wrightstown, Burlington County. A further investigation demonstrated the fact beyond a doubt. The total loss in the herd was nine animals.

On the 14th of September I visited Salem County, in answer to a report that Texas fever was carrying off the cattle in a locality in that county. The result of the investigation was very unsatisfactory, as the owners of the animals were very reticent concerning the matter. Sufficient evidence was obtained, however, to locate the disease and ascertain its character. Several deaths had already occurred, and five animals were found sick at time of visitation.

On the 9th of October I received instructions to report at once to your Department for further orders. I arrived there on the morning of the 10th, and was instructed to proceed to Rappahannock, Va., and examine the cattle on the farm of M. J. Wine, at that place. I arrived at the farm on the morning of the 11th and examined what animals were still upon the premises, all of which were in good health. An investigation of the facts concerning the disease that had existed on the farm led to the conclusion that contagious pleuro-pneumonia was its character. Several deaths had taken place, and the balance of the infected herd had been shipped direct to Baltimore stock-yards (a few days prior to my visit) and there sold. A special report of this case was made to your Department at the time of visitation.

On the 18th of October I received an order from your Department to at once proceed to Sterling, Whiteside County, Ill., and investigate the character and extent of a disease existing among cattle in that vicinity. I started forthwith, and arrived at Sterling on the morning of the 20th. Accompanied by Dr. M. R. Trumbower, I visited the several localities where any disease was known to exist, and acquainted myself with all the circumstances in connection therewith. The disease, which was splenic fever, was found upon several farms, but at the time of my visit was rapidly decreasing, and from the appearance of the animals seen I have no doubt that it had passed its dangerous stage in most cases, as all the animals appeared to be convalescent. I made visitations to other sections of the State, in order to ascertain whether or not this disease

was existing in other localities, but as far as I was able to ascertain there had been no extensive outbreak of it. A special report made to your Department soon after my return contained a full account of my investigations. Accompanying this you will also receive a copy of a paper written by Dr. M. R. Trumbower, of Sterling, who had charge of all these infected herds, and who made daily visitations to them during their sickness. At my solicitation, he has forwarded to me a copy of his report of the same, and as it is a very interesting and instructive report, I respectfully forward it to you for publication should you so desire.

To the diseases of other domesticated animals I have given but little attention while investigating those of cattle. There have, however, been several cases of glanders in horses that have come under my immediate observation during the past year, all of which were reported to the local or State boards of health.

Epizootic influenza has been quite prevalent in some sections of this State, and is existing at the present time. Swine plague has existed in some portions of the State, but not to so great an extent as last year. Chicken cholera has also carried off many flocks of poultry, but this, too, has not spread over so large an extent of the State as in previous years, and poultry raisers are beginning to hope for better times.

I have the honor to be, very respectfully, your obedient servant,

WM. B. E. MILLER, D. V. S.

CAMDEN, N. J., *January 30, 1883.*

NOTES ON AN OUTBREAK OF TEXAS FEVER AMONG CATTLE.

BY M. R. TRUMBOWER, V. S., STERLING, ILL.

September 24, 1882, I was requested by Mr. S. F. Gleason, of the firm of Martin & Gleason, dealers in cattle, to go with him to examine several animals belonging to them which had been found dead in the field.

The first one examined was a two-year-old steer. Found him lying on his right side; had probably been dead six hours; ground indicated no ante-mortem struggles; feces passed, thin, green, and well digested; nostrils clean; membranes of a pale gray color; no evidence of bloating or decomposition.

Removed the left shoulder, cut through the ribs at their superior articulations, and removed entire the walls of the chest and abdomen, exposing the internal organs to view. The lungs were found healthy, with the exception of a slight emphysematous condition and trifling hypostatic congestion of the posterior lobe of the right side; pericardium of a normal appearance—contained about an ounce of bloody-colored serum; heart pale, flabby, and of normal size; externally the apex, right side, and both auricles presented slight ecchymosis; the left ventricle contained a small quantity of thin, watery-appearing blood, but no clot; the columnal carneæ were covered with petechia of a bright, purplish tint; the right ventricle contained a small blood clot; the valves, chords, and columns were healthy. Examining the abdominal organs, the spleen attracted immediate attention; of a dark-purple color, enlarged to three times the normal size. Incising it, the trabecula were found broken down and a general disorganization had taken place; liver somewhat enlarged, the upper

portion of right lobe of a straw color; gall bladder contained about six ounces of semi-fluid, viscid, almost brown-colored bile.

First three stomachs indicated no functional nor organic disturbance; fourth stomach, lining membrane reddened and having an irritated appearance; the color presented a few ecchymosed spots on its external surface; the internal surface of jejunum slightly congested and thickened; the mucous membranes of cæcum and rectum also manifested some congestion. Kidneys somewhat enlarged and of a dark color in their circumference, otherwise apparently normal in structure and contents; urinary bladder perfectly healthy in appearance, and contained about four pints of very dark-colored urine; specific gravity of urine 1008, slightly alkaline, and formed a large coagulum upon boiling.

The appearance of the muscular tissue of the animal was very pale and bloodless; the small amount of blood contained in the arteries and veins was of the same color, very thin and watery, and, from a total absence of clots, was apparently almost entirely destitute of fibrin.

Post mortem examination of steer No. 2: Aged two years; died apparently without a struggle; still quite warm. Proceeded as in No. 1; found the appearances throughout about the same, except a considerable congestion and apparent subacute inflammation of the intestines; the longitudinal folds of the rectum presented black streaks, caused by an extravasation of blood under the mucous membrane; contents of rectum thin and mixed with blood and mucus. Mesenteric glands greatly enlarged, some of them measuring fully 3 inches in length, 2 in width, and half an inch in thickness, of a vealy color, and possessed a soft, doughy feel to the touch.

These two animals examined were in but fair condition, and had the appearance as if rapidly fallen off in flesh; gaunt, and the skin drawn tight over the ribs.

Two more steers belonging to the same parties were reported as lying dead on the other side of the creek; but it was getting late in the day, so they were not examined. They had died some time during the previous night.

The two examined were alive in the morning; one of them down, unable to rise, and the other one staggering in his gait and purging. This was the first intimation had of any sickness among the herd of Martin & Gleason's cattle, and the first ones that died in their field during the season.

Upon leaving the field I noticed two animals showing evidence of sickness, manifested by weakness—a general loose-jointedness in their gait, saliva dribbling from the mouth, eyes dull and staring, and a diarrhea. After considerable chasing, we succeeded in catching one; age, two years; pulse, 90; respiration, 32; had been chased half an hour; legs, horns, and body warm; administered one dram of sulphuric acid in water. Next day Mr. Gleason reported this one apparently improving, and the other one worse; but on the morning of the 26th both were found dead, together with a third one which had not been previously noticed. No opportunity was given for examination. Martin & Gleason suffered a total loss of 7 head out of 130 head then in the pasture. Six of the number that died had been placed upon the field on the 8th day of September, and the seventh one had been in since early in June.

The field in which these cattle died is situated 4 miles north of Sterling, Whiteside County, Illinois, and on the west side of a public road running north and south. The field contains 160 acres of part timber and part bottom land. Through it runs a good-sized stream of water, named Elkhorn Creek. Into this field were placed 100 head of Cherokee or Indian cattle on or about the 5th day of last June, together with about 50 head of native cattle. On the 23d day of July another lot of Cherokee and native cattle, numbering 50 head, were added. Both lots were purchased in Chicago, where they had just been received from Saint Louis.

Throughout the season, or from June 1 to September 25, about 500 head of cattle or upwards, including the two lots mentioned, had been pastured on this field, and from thence went by small lots to the butchers, at the rate of 100 a month.

On the 25th of September about 120 head, the number then remaining in the field, were sold and shipped to Chicago.

On the 1st day of July a heavy freshet occurred, which swept away the fence intervening between the field of Martin & Gleason on the west, and that of Mr. William Ecternach on the east; the fences along the road were also carried away, and the cattle of Martin & Gleason crossed the road into the pasture field of David Wolf; through it into John Stronffer's; thence into Benjamin Bressler's timber. In Mr. Wolf's field some of the Cherokee cattle remained for several days, and one until the 6th of October. The field belonging to Mr. Ecternach, traversed by the cattle of Martin & Gleason, was partly planted with corn and part sowed in oats. Mr. Ecternach's cattle did not come in contact with Martin & Gleason's cattle after the 2d of July, neither did Martin & Gleason's cattle penetrate into Mr. Ecternach's regular pasture field, which adjoined the cornfield on the west. The Elkhorn Creek runs through all these fields above mentioned, its course being from east to west.

October 8, Mr. Phares Landis, living 2 miles north of Sterling, informed me that he lost a steer on the night previous. I proceeded to his place, accompanied by Messrs. D. N. Foster and Henry Bressler, the latter gentleman informing me that he also lost a fat steer this morning.

History given by Mr. Landis: Removed from David Wolf's pasture field 12 head of cattle on the 5th, where they have been since the 1st of May; on the 6th one of the animals brought home was noticed to be ailing, and on the morning of the 8th was found dead.

Post mortem appearances No. 3: Aged thirty months; fat and in excellent condition; lying on his left side, outstretched; drops of blood standing all over the body; turned him over on the right side; removed the left shoulder and ribs; found very slight congestion of posterior lobe of left lung, and some interlobular emphysema; pericardial fluid bloody-colored; external appearance heavily congested, almost a black color; small blood clots in both ventricles; weight 4 pounds. Spleen, weight $4\frac{1}{2}$ pounds, presenting an engorged and disintegrated glandular structure. Liver, weight $19\frac{1}{2}$ pounds, very dark-colored, blood vessels filled with blood; evidence of a fatty degeneration left upon the knife blade after making a number of incisions into its structure. Fourth stomach—internal surface congested and studded over with a number of small circumscribed abrasions—dark-colored in the center and deep; discolorations found throughout the intestinal canal; longitudinal folds of rectum black-streaked. Urinary bladder healthy, containing about two quarts of very dark-colored urine, specific gravity 1010, coagulated upon application of heat and nitric acid. Kidneys weight, right, $1\frac{1}{2}$ pounds; left, $1\frac{1}{2}$ pounds—both kidneys containing a little bloody-colored urine, the cortical portion darker than normal—a dull-brown in color. Muscular tissues pale and almost bloodless, presenting the appearance in color of veal. Blood in blood vessels watery, but darker in color than in cases Nos. 1 and 2.

October 8.—A steer owned by Henry Bressler, 5 miles northeast of Sterling, died last night. This steer, with 15 more belonging to Mr. Bressler, had been in Mr. Wolf's pasture from the 20th of May until the 5th of October; October 6 was noticed to be sick by one of the neighbors; was found dead, lying in the creek (not Elkhorn), in Mr. Bressler's field on the morning of the 8th.

Post-mortem appearances No. 4: Aged three years; proceeded to examine as in the previous cases; muscular tissue rather dark-colored and presenting a dark speckled appearance; blood vessels containing a normal amount of blood; blood clots very strong and abundant; lungs emphysematous and considerably congested; a slight deposit and organization of lymph, forming a small adhesion of the right lobe to the diaphragm; pericardium contained about an ounce of bloody-colored serum; heart flabby and extensively ecchymosed on its external surface; contents, heavy blood clots in each ventricle, extending through the valves into the auricles, the fleshy pillars of the right side almost black. Spleen, weight 5 pounds, ecchymosed; engorgement of the blood vessels of the covering peritoneum; trabeculae broken down.

Liver: Weight, 24 pounds; superior lobes an olive color, lower lobes very dark and somewhat hepatized; gall bladder contained about 12 ounces of brown-colored bile, and of a granular appearance. Abomasum: Internal surface covered with small papillar eruptions, and several small ulcers with ragged edges; small intestines red-dened and congested throughout their length, and occasionally presenting groups of small ulcers; there was also considerable softening of the mucous coat of the intestines; contents of rectum covered with mucus and small blood clots. Urinary bladder healthy, containing about 12 ounces of grape-colored urine. Kidneys same as in previous cases. This animal was fat, and had apparently not lost much in condition.

Mr. Bressler also lost one of his steers in Mr. Wolf's pasture about the 15th of September; was found lying dead in the Elkhorn Creek, where he yet remains (contrary to my orders), and another one—a heifer—died a few days later. Total loss occurring in Mr. Bressler's herd of 18 animals, 3.

Mr. David Wolf lost three head out of a herd of 80 animals, about the same time that Mr. Bressler's second one died. Ben Bressler, in the adjoining field, lost a cow and a two-weeks old calf, about the 23th of September; ten head exposed.

John Stouffer found two steers dead about the 1st of September after missing them for a week. October 10, I was called upon by Mr. William Echternach to see some sick cattle, 4 miles northwest of Sterling.

History.—His cattle had been grazing all summer upon a field through which flows the Elkhorn Creek, after passing through the fields of Martin & Gleason, David Wolf, and others. Since about the first of August they had been herded a part of each day on the field adjoining, over which field the Cherokee cattle had been roaming at the time of the flood, July 1.

On arriving at his place I found five head of cattle penned up in a yard, and one was left lying in the lane unable to rise.

No. 1.—A roan cow, four years old; was first noticed to be dull the day before, and had diminished in the yield of milk; this morning had only yielded one-half the usual quantity. I found her with her head and ears drooping; nose dry; horns warm; eyes dull and rather prominent; pulse 80; respiration 26; visible mucous membranes of a natural color; eats and remasticates.

No. 2.—Roan cow, twelve years old; in the seventh month of pregnancy; not being milked now; pulse 96; respiration 26; membranes pale; don't eat.

No. 3.—Red cow, six years old; yielded this morning only one-half the usual amount of milk; appearance wild and excitable; pulse 66; nose moist; horns cold; eats and ruminates; feces very dark colored.

No. 4.—Red heifer, one year old; pulse 76; respiration 26; horns cold; nose dry; very tender along spine; trembling violently.

No. 5.—White steer, one year old; pulse 72; respiration 20; stiff in gait; feces coated with mucous.

No. 6.—Red heifer, two years old; unable to rise; tremor of flank muscles; membranes of a decided yellow tinge; breath hot and fetid; feces covered with small clots of blood; urine bloody-colored; nose extended; the lower jaw resting on the ground; indifference to surrounding objects and noises; pulse 120; respiration 40. Treatment prescribed:

R Acid carbolic.

Soda bi-carbonas $\bar{a} \bar{a} \frac{3}{4}$ vi.

Glycerine $\frac{3}{4}$ iii.

misce.

Signa: Half an ounce to be administered to each animal, in a quart of water, every six hours. Feed to be given: Green corn stalks and boiled oats, with a little salt in each feed. I had them placed in a pen under a shelter, the yearlings separated from the cows. No. 6 was left where she then was.

The remainder of Mr. Echternach's cattle were placed upon another field, removed

from where they had been October 11. No. 1, pulse 70; respiration 18; nose dry; membranes of a decided yellow tinge; eyes prominent; horns cold; legs cold; drooping of head and ears; remasticates; feces very dark colored. No. 2 lies down most of the time; hair erected and dead looking; horns hot; nose dry; eats but very little; shows evidence of suffering; very hollow in the flanks; feces very dark colored; pulse 100; respiration 24. No. 3, still excitable; horns warm; nose moist; legs cold; pulse 56; respiration 14; feces almost black; gives one-half the usual quantity of milk. No. 4, pulse 68; respiration 20; horns very warm; legs cold; nose dry; eats a little; don't remasticate and refuses to drink.

The only thing I can notice abnormal in No. 5 is a disinclination to move. Same treatment to be continued; dose to be given every four instead of every six hours. No. 6 died about 11 o'clock, a.m.; was found within 4 feet of where she was yesterday at 4 p.m.; lying on the right side; small passage from the bowels is harder than normal; a little blood and mucus adhering to the same; a few drops of blood hanging to the hair on the inside of the thighs. Removed the ribs and abdominal wall of the left side; found a small quantity of bloody serum in the thoracic and abdominal cavities; lungs presented less congestion and emphysema than any yet examined; pericardial fluid bloody-colored; considerable ecchymosis over the left auricle of the heart, and also on the apex of the corresponding side; no heart clots whatever; blood very scanty, thin, and of a bright scarlet. Spleen, weight three pounds and a half; found an extravasated blood clot on the superior border, and under the peritoneal covering, 2 inches in width and 4 in length, caused by a slight rupture of the capsule of the organ.

Liver: Weight, 13 pounds; upper portion of lobes lighter in color than normal, and presenting evidences of fatty degeneration; bile in gall-bladder a greenish-brown and ropy; kidneys normal; abomasum, internal coat studded over with granular-looking pointed elevations, and the cardiac end congested and softened; mucous membrane of small intestines reddened and somewhat softened; urinary bladder filled with urine the color of port wine; specific gravity 1012; coagulated by application of heat; mesenteric glands slightly enlarged; uterus healthy, containing a three-months-old foetus, which had several radiated ecchymosed spots about the head.

No. 1.—October 12, pulse 52; nose moist; horns warm; eats, drinks, and remasticates; rumen very hard, almost impossible to indent it with the fist; feces still too dark colored.

No. 2.—Pulse 76 and soft; eats, drinks, and remasticates; feces still too dark in color.

No. 3.—Pulse 52; lost the excitable appearance; eats, drinks, and ruminates; feces assume the natural color.

No. 4.—Pulse 60; don't drink; eats and ruminates; appears uneasy and don't lie down.

No. 5.—Apparently all right.

Ordered Nos. 3 and 5 to be turned out again; the rest to be treated as before, with the addition of ferruginous and vegetable tonics.

October 15.—No. 1 does not appear entirely well yet; feces of a lighter color and softer than before.

No. 2.—Filling out rapidly; feces of a normal appearance.

No. 4.—Pulse 86; respiration 26; nose moist; don't eat, drink, nor remasticate; persists in lying down; very tender along spine; constipated; manifests stupor and indifference to surroundings; apparently suffers no pain; coughed once to-day for the first time.

No. 7.—Red heifer, two years old; brought up yesterday; pulse 66; respiration 20; nose moist; horns warm; saliva flowing from the mouth; eats very little; weak and not inclined to move; manure very dark-colored and hard.

No. 8.—Steer, one year old; brought up yesterday; noticed to be sick on the 13th; passed bloody urine last night; died to-day at 2 p. m.

Post-mortem appearances: Was thin in flesh; upon removing the right shoulder areolar tissue was found filled with gas, forming small bladders or bubbles; muscular tissue pale and almost bloodless; lungs presented nothing abnormal; the trachea and larger bronchii contained some white frothy sputa; heart ecchymosed over external surface; internally destitute of blood clots, and the small amount of blood contained was very thin and watery; slight petechia in left ventricle; spleen, weight $3\frac{1}{4}$ pounds; internal structure broken down; abomasum presenting several small ulcers and abrasions, and a general redness of the mucous coat, as did also the small intestines; liver, weight 13 pounds; gall-bladder contained about 3 ounces of very dark colored bile of a granular appearance, similar to old rancid lard; urine bladder contained about 20 ounces of a dull, red-colored urine; hard balls of feces, coated with blood and mucus, were found in the blind end of cæcum.

October 20.—No. 1, pulse 56; gaining in milk; No. 2, pulse 54; No. 4, pulse 63; No. 3, yielding her full quantity of milk. In the afternoon I made another visit, accompanied by Dr. Miller, who concurred with me in pronouncing them all convalescent.

October 24.—No. 2 aborted, otherwise doing well.

On the 3d day of October Mr. Echternach found a yearling heifer dead in his pasture field; making a total loss of three, and six recoveries in the herd of 40 animals exposed.

A tabulated statement of the average temperature and rainfall of this section, kindly furnished by Mr. Samuel A. Maxwell, of the Northern Illinois College, also Volunteer Observer, Signal Service, United States Army:

Months.	Average temperature.	Average rainfall.
1880.	° F.	Inches.
June.....	73.37	5.74
July.....	75.68	3.35
August.....	75.55	4.15
September.....	61.45	4.15
1881.		
June.....	68.27	9.79
July.....	76.18	2.39
August.....	76.43	1.75
September.....	69.29	3.90
1882.		
June.....	68.38	10.06
July.....	70.31	3.50
August.....	71.20	1.81
September.....	62.60	1.48

STERLING, ILL., December 15, 1882.

CONTAGIOUS PLEURO-PNEUMONIA IN MARYLAND.

REPORT OF W. H. ROSE, D. V. S.

Hon. GEO. B. LORING,

Commissioner of Agriculture :

SIR: In compliance with the request contained in your favor of August 31, 1882, I take pleasure in forwarding the following supplemental report of investigations of the different herds of cattle in the State of Maryland affected with contagious pleuro-pneumonia. During the winter months of the present year, I devoted the most of my time among the slaughter-houses and stock-yards, with a determination to examine the lungs of all cattle that were killed, especially those that came from the Western States. The fact that but few Western cattle are killed in this city, limits my report to a few hundred in number. However, I have failed to detect a single case of this disease among them, either in life at the stock-yards, or among the *post-mortem* examinations which I have so often made in the slaughter-houses. The only lesions found were an occasional case of adhesion of the pleuro, or emphysema, and a very few cases of bronchitis. The latter I have attributed to the past mild winter. In speaking of the cattle in general that are slaughtered in Maryland, especially about the city of Baltimore, I have very little to say, because of the many small places in all sections of the city and county where I find it a difficult matter to enter, having no power to force an entrance when rejected. Occasionally I have detected cases that were sent to some of these places, and I know that many others have gone the same way, for I have missed them from their positions in the infected stables. I would not feel alarmed if this was the result with all cases, but we are not safe in accepting such a proposition, for there are many dealers in live stock who frequent those stables and make exchanges with the owners of infected animals for healthy ones. (See report of Baltimore city and vicinity.) I have made repeated visits among these stables, which are situated in all parts of the suburbs of this city and county, and I cannot discover any other cause for the perpetual transmission of the disease throughout Maryland and adjoining States than the constant exchanging of cows by the owners of these stables. Dealers often visit these localities with small herds of healthy cattle brought from other counties where no contagious disease ever existed, and they are sold or exchanged for diseased animals. Many of them, even if they have not suffered from the effects of the disease, are infected. No action has been taken by the authorities of the State to prevent the removal of animals from

these infected premises, nor have any regulations whatever been brought to bear on the introduction of healthy animals into infected stables. In this way fresh virus is constantly being supplied, and the disease continually propagated. If healthy animals alone found their way into our stock-yards, the matter would not be of so much consequence; for we could prevent to a great extent the removal of infected animals from Baltimore, into the States of Pennsylvania, Delaware, and New Jersey, as well as the surrounding counties of Maryland; but how often do diseased animals either accidentally or intentionally, so far as their owners are concerned, gain entrance there? I cannot help expressing the opinion that the greatest error to be found lies in the fact that these infected stables are not watched by the State authorities, nor do they compel the owners of all such stock to send them, if sick, to the offal dock. The balance of the herd, even if have never shown any symptoms of the disease, should be sent directly to an abattoir and killed as soon as the powers of lactation have subsided. This would prevent the venders of stock in this city from spreading the germs of destruction to all quarters.

Regarding the use of prophylactics, I have yet to see any precautionary measures adopted by any of those interested about Baltimore, and I feel satisfied that were inoculation practised upon all animals that enter these stables, and the infected ones killed, we would be working at the root of the evil.

During my investigations this year throughout the different counties of Maryland I have not found many fresh outbreaks of the disease, nor have I detected many cases among the dairy stables surrounding the city, but enough has existed in the latter places to create most of the new outbreaks in the counties.

NORTHWEST OF BALTIMORE CITY.

During the winter months some of the dairy cows owned by James Vaughn, situated a few miles north of Woodberry, Baltimore County, died of contagious pleuro-pneumonia. Up to April 12, 1882, seven of them died and three others were suffering, while several more had convalesced. I gave you a detailed account of this place in my last report, and it will be out of place to enter upon a repetition of the facts here. I have never found so many cases before on this place at one time, which is due entirely to the controlling influence exercised by the Hanover Animal Life Insurance Company, which will not pay for the animals until they are dead. Thus you can plainly see many of them are kept from the stock-yards. I have noticed that the affected cows are to be found mostly in the older part of this stable, which has been standing ever since the first outbreak, while the remainder is entirely new. This I consider a very important point, and illustrates the infectious nature of the disease. Exchanges are made at this place with dealers for other cattle.

David Stevens, also of Woodberry, lost four cows affected with the same disease during the winter months. I have found none during this summer. Some of his cows were sent into the stock-yard in the incubative stage of the disease during the past season. No doubt the same practice is continued. (See report of last year.)

EAST OF BALTIMORE CITY.

I have visited all of this section during the summer months, and I have found very few acute cases among the different herds. One remarkable feature that I have noticed this season among the majority of the stables is that the virulence and spread of the disease has declined for a period. I have noticed this fact particularly among some of those where its destructive elements were lodged in the early part of last year. Still many cases no doubt have existed unknown to me in all these localities, as I have no authority to enter these stables if objection is made.

On the farm situated near the Belair road, and occupied by Mr. Jeokel for dairy purposes, may be seen at any season of the year one or more acute cases of this disease. Especially is this the case among new arrivals after they have remained long enough on the place to show the symptoms. Many cases have developed on this place during the past few years. The owner admits that 300 head have sickened and many of them have died, while the rest are oftentimes kept until they become profitless, and then they are exchanged or sold. I consider this stable infected with the most virulent form of the disease.

SOUTH BALTIMORE.

This section of the city has several infected stables where many cases of disease have developed in past years. I always expect to find more or less of it among them while they are left standing. Some owners isolate (?) their stock from those that are suffering from its effects in the following manner: The sick cow is turned out on the commons to roam at will, while the remainder of the herd is allowed to pasture over the same ground, separated only by keeping them apart. Most of the herds graze on these commons.

The herd belonging to Mr. George Sachs, of Hanover street, I consider in a very precarious condition. He has tried very hard to check the destructive progress of the disease without success. Its malignancy is due entirely to the amount of virulence within his stable. Single cases have continued to develop during the past summer. Up to August 8, 1882, I had found two cases in this herd. One of these was very low, while the other contracted it in a mild form. Not far distant from this place, situated on the same street, can be seen another herd of dairy cows belonging to Mr. Hiller, where the disease has developed itself in the same manner during the present summer. In the latter part of June one of his cows died on the commons, while another died in the month of July. I have omitted giving a description of

many stables surrounding the city of Baltimore, on account of so few cases having developed in them this season.

REPORT OF BALTIMORE COUNTY.

The only outbreak in this county this season I found located near Whitehall, on the Northern Central Railroad. The cattle were owned by Christopher Slade, an ex-member of the State legislature. They were infected by a cow purchased from Chas. L. Almony & Brothers, who live $4\frac{1}{2}$ miles east of Parkton (this county) and 3 miles south of the Pennsylvania line. Mr. Slade has been very honest in the confessions which he has made at different times, not only of the number infected, but as to the disposal he made of some of them prior to his knowing the nature of the disease. During the early part of March this cow remained on this farm for three weeks, before he noticed any change in her condition; and even then he did not consider her in any danger until very severe symptoms were observable, when he was led to believe her dangerously sick. He did not isolate her from the rest of his herd until it was too late. He did not suspect that such a disease was lurking among his cattle until a second cow was taken sick. On April 29, I made my first visit to this farm, and during the interval, between the first case and my visit, two animals had died, and three others which had developed the disease were isolated from the herd. Their temperature varied from 107° F., $105\frac{1}{2}^{\circ}$ F., $103\frac{2}{5}^{\circ}$ F. Two were affected in the left lung, and one in the right. At this time his herd of 24 animals were divided in this manner: Nine of them, which he considered in perfect health, were sold to a dealer named Adler, who drove them along the turnpike road for a distance of 28 miles, in order to sell them at the Calverton stock-yard. In trying to house them over night at one of the roadside barns, admittance was refused by the owner of the place. Nine others were kept by Mr. Slade, and they were kept separated from the sick cows by keeping them in a pasture field day and night, where none were sick. I noticed no case at the time of this visit among those nine animals. I advised the killing of the three sick cows, which was done after I left the farm. During the month of May several cows were brought from a farm not far distant from this one, both belonging to Mr. Slade. Three of these new arrivals died from the effects of the disease. Since this last loss there has been a rest for a few weeks. On June 14 I found a case which was almost imperceptible, the temperature being the only guide. Two chronic cases that had been suffering for a few weeks were also found.

On June 1 a report was given to the press of Baltimore, signed by the governor and his veterinarian, that no contagious pleuro-pneumonia had existed in the State of Maryland for the last six months, nor could a case be found at the present time. In honor of your Department, and for the welfare of the stock breeders in general, I made it a part of my duty to contradict this statement, not only in the local

papers, but in the August number of the American Veterinary Review. This aroused the suspicions of the members of the United States Treasury Cattle Commission. Professor Law, a member of the commission, wrote asking me where and when I could show him some cases of disease. Having gained permission from the Department to accompany the members of this commission to the Slade farm, Professor Law, Dr. Thayer, and myself visited this place June 19, and, with a careful examination, five cases were found with varying temperatures, ranging from $101\frac{4}{5}^{\circ}$ to 106° F. We found two newly-developed cases since my visit of the 14th instant. One of these cows was purchased by Professor Law. It was killed, and a portion of the right lung was taken to Washington and shown the Committee on Appropriations. On the same day, previous to visiting Mr. Slade's, I went with these gentlemen to the farm owned by Charles L. Almony, about 4 miles north of Mr. Slade's, and only 3 miles from the Pennsylvania line. Here we found a calf that had been purchased by Mr. Almony from John P. Mays, of Hereford (same county), where many fine cattle had died last year from this disease. We found no trace of this disease about this young animal, and I cannot believe that the transmission was produced by this calf unless by infection. However, two cows died here in the latter part of last winter, and one or two others were slightly ill, but they convalesced very rapidly. I am perfectly satisfied that Mr. Almony knew nothing about this disease at the time he sold Mr. Slade the infected cow, nor could he ascribe any other cause for its origin on his place than the introduction of this calf, until I made a visit to some of his neighbors who had lost cows during the past winter. Two of them, who live on the turnpike road leading to Newmarket, Pa., named, respectively, Brown and McCullough, acknowledged that they had lost animals by the disease. The brown cow was turned out to die, but she rallied without medicine, and he sold her to a man named Waldron, who removed her to Newmarket, Pa. Mr. McCullough lives opposite to this place, and he lost two cows before Mr. Almony lost any of his. The two latter herds have mingled together, which I consider the principal cause of transmission. But this was not the only cause for infection. Cattle are removed from Baltimore to Pennsylvania along this pike at different seasons of the year. On July 26 I made my last visit to the Slade farm, and found only three cows remaining. The remainder of this herd, which showed no symptoms of the disease (9 in number), were killed and dressed on the place by a butcher, who purchased them at \$20 per head. The sick cows were killed and buried. Professor Law sent three calves to this place from New York, inoculated with the virus of the disease, with the express purpose of studying the effects of inoculation in the midst of a very virulent form of the malady, but the entire herd was destroyed, except the three unaffected animals, previous to their arrival.

During the month of July, cattle were taken sick in different parts

of this neighborhood; but I saw no traces to indicate any suspicion for the existence of contagious pleuro-pneumonia, except in a herd belonging to John M. McComas, jr., near Whitehall. On July 26 I found a cow on this place that came from the McCullough farm. The only symptoms shown were a loss of appetite and diminution in the amount of milk. She was isolated from the rest of the herd with two others, which were with her constantly. However, no symptoms have developed among them as yet, but we must still look upon them with suspicion.

On May 27 I visited a small stable in Towsontown, belonging to Mr. Parlett. I reported this place to you last year as being infected, and I discovered that two cases existed there this year. One of them was sold during this month, but he could not tell me where she had been taken; the other case has recovered. This man is related to Mr. Wisnor, who had the outbreak last year on the Hillen road. They make use of no restrictions, and both of them claim to have a specific for the disease.

RECOVERED CASES IN BALTIMORE COUNTY.

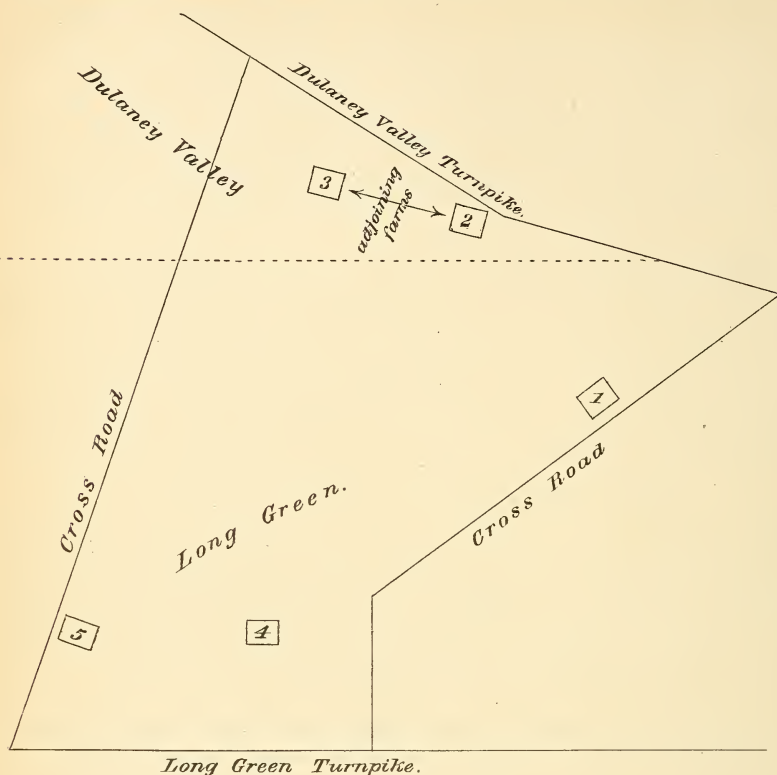
I have paid particular attention to the movements of such animals during the past season, especially during warm weather. In the locality known as Long Green and Dulaney Valley many recovered cases can be found, but where very little transmission has occurred on account of so few new arrivals coming in contact with them.

The cattle owned by General Trimble, in fact most of the infected herds, are insured by the Animal Life Insurance Company which allows them to keep the recovered cases for their future safety from the disease, and willingly pays for those new arrivals that may die from its effects. A cow has died occasionally on the Trimble farm, but no new outbreak has occurred in this locality. This is due entirely to the honesty exhibited by most of the owners of these animals, who will not allow them to leave their farms. This I know to be a fact, for I have them numbered on each farm, and not only in this locality but in other parts of the same county. I have advised their destruction, but this the owners will not do. I will not go into details concerning these herds, and hope the following summary will convince you that great destruction of cattle would occur if these 37 animals were distributed to all sections of the country in addition to the number sent out from Baltimore dairy stables:

Recovered cases.

General Trimble, Dulaney Valley,	10
Thos. Pierce, farm adjoining	4
Jno. A. Conklin, Long Green	6
Joseph Dilworth, Long Green	4
Jno. P. Mays, Hereford, near Monkton	2
Alfred Mays, Hereford, near Monkton	3
Judge D. M. Perrine, Charles street avenue	7
J. B. Manning, Charles street avenue	1
Total	37

Diagram of infected farms at Long Green and Dulaney Valley.



No. 1. John A. Conklin's, 6 recovered on his farm.

No. 2. General Trimble's, 10 recovered on his farm, 15 died.

No. 3. Thos. Pierce's, 4 recovered on his farm, 7 died.

No. 4. Joseph Dilworth's, 4 recovered on his farm; unknown.

No. 5. John Wilson's, none recovered on his farm, 5 died.

BALTIMORE STOCK-YARDS.

I have made weekly visits to these places, but have found very few cases of contagious pleuro-pneumonia of the acute type this season. However, I am satisfied that quite a number of mild cases have escaped my observation for lack of proper opportunity to make examinations. I have noticed many suspicious looking cases that I was not allowed to examine that were sent to Philadelphia, Wilmington, and other places unknown to me. It will be necessary for me to illustrate the movements of cattle about these yards, before you can thoroughly understand the great danger which accompanies the purchase of any animal within its pens. I speak particularly of the Calverton stock-yard, where so many infected animals have carried the germs of disease to

* The dotted line separates Long Green from Dulaney Valley.

all parts of the surrounding country. On Monday of each week cattle from the upper counties of Maryland, also Virginia and other Southern States, as well as Texan and Western stock, are sold at this place. They are at first kept in separate lots by the dealer who enters them for sale. Butchers from Baltimore and dealers from other States, as well as the owners of common stock in the different counties of Maryland, make the purchases in the following manner: They visit each herd of cattle and select a few from each lot, after which they are placed in one pen and are soon scattered in all directions. Some of them are driven to the new stock-yard, recently built by the Baltimore and Potomac Railroad Company, where they are watered and rested and then loaded on cattle trains for Philadelphia and elsewhere. Most of the infected animals are brought into the Baltimore stock-yard by the dealers from Baltimore City, and a few are sent from the farms in Baltimore county where the disease exists. The Mount Clair stock-yard, which is situated a few miles southwest of Baltimore, and owned by the Baltimore and Ohio Railroad Company, has been built regardless of expense in order to meet the demands for the safe transit of cattle to be shipped to all parts of the country. The yard is thoroughly isolated from all sources of infection, unless brought there by Baltimore cattle. In conversation with Mr. Anderson, the private secretary of President Garrett, of the Baltimore and Ohio Railroad, I advised him to exclude all cattle from infected localities, and judging from the response given I am satisfied that all such stock will be rejected. I have noticed many herds of Western cattle that have rested at this yard during the present season, and I have yet to find any Baltimore animals mingling with them. They will continue to remain safe, if care and attention to this restriction be given. The question has been asked, why do so few of the thoroughbred herds of Maryland experience a loss from the effects of this disease? In answer to this question I may simply say that the only thoroughbred cattle that ever pass through these yards are either deformed or below par from some cause or other, and the latter order of stock rarely come in contact with the former.

During my investigations at the Calverton stock-yard I have detected acute cases during the following months: April 24, one acute case; May 22, one acute case; May 29, two acute cases.

During the month of July, a herd of Texas cattle were driven from the Calverton stock-yard along the Powhatan road. They had traveled but a few miles, when they commingled with a herd of Maryland dairy cows, which very soon developed the disease commonly known as Texas fever. On the 8th of August every cow was dead. This herd consisted of eight cows, and was owned by Mr. Hush, who lives near Powhatan, Baltimore County. No other cattle in this locality developed the disease, although a line fence was the only means of separa-

tion between them and two Maryland herds. The Texans were killed a few miles from this place.

MONTGOMERY COUNTY.

On July 7 I examined a herd belonging to Wm. M. Moore, of Sandy Springs, which was affected with this disease. The governor had been notified of this fact by the owner some time before my visit, who was desirous of preventing its spread, not only among his own cattle, but among those of his neighbors. The State veterinarian was sent to this farm by the governor, who gave him power to quarantine the place, and try to prevent its spread.

During my visit I found the herd separated; 14 of them had never come in contact with the first case. These animals had been sent to an adjoining farm, and were milked and fed by men who kept away from the other cattle. The remainder, 12 in number, were held with suspicion, and were watched closely from the month of April until the month of August. Four cows have developed the disease at different periods, varying from three to four weeks between each case. These were destroyed.

I found no traces of the disease on the farm of John E. Diamond, near Gaithersburg, where it was reported to have existed. A few calves died early in April, while the cows remained in perfect health up to the time of my visit. On July 20 I visited the farm owned by Thos. J. Lea, from whom I received the following facts: The first animal attacked on Mr. Moore's farm was purchased from Messrs. Lea & Gilpin, Baltimore cattle dealers. She remained on the Gilpin farm two weeks before Mr. Moore bought her. Three weeks after arrival on the Moore farm, symptoms of the disease were observed. Mr. Lea purchased a bull of Mr. Diamond a short time afterwards and he developed the disease by standing in the stall which had been occupied by the Baltimore cow. He died. No other cases have developed on the Gilpin or Lea farms.

WASHINGTON COUNTY.

In the months of May and June, 13 head of cattle died in the small place called Ponsville, 8 miles east of Hagerstown. This section of the county is very mountainous, and the vegetation very poor, which compels the cattle to enter the woodlands and feed upon anything which is agreeable to their taste. They often partake of astringent plants whose properties are deleterious to their health, especially if indulged in too freely at one time, or continually taken into the system as provender. Some of the owners were sure that contagious pleuro-pneumonia had made its appearance among their cattle, and they notified Governor Hamilton, who immediately took steps to investigate the matter. The disease proved to be a low form of anthrax, associated with sore throat. On August 12 I visited Hagerstown, and in company with Dr. Cosens,

an English veterinary surgeon, who has had considerable experience with the lung plague in England, visited the principal stock farms in this locality, but failed to find a single case.

Through the advice of Dr. Cosens, I visited a place called Hancock, in the same county, where, I had been told, cattle were dying. I failed to detect any sick stock in this locality, but I learned here that a number of cows were suffering from some disease in the State of Pennsylvania, near the State line.

CARROLL COUNTY.

On April 28, Mr. W. H. Westaway, of Patapsco, purchased a cow from a dealer named Henry Warner, who brought her from Hanover, Pa., a place where contagious pleuro-pneumonia has existed for some time. No symptoms of the disease were shown in this case until after parturition, which occurred in the first week in June. Even then the owner was not alarmed. On account of its slow development no restrictions were enforced, and the animal was allowed to roam all over the place, until the month of August, when another cow was taken sick. The owner then notified the State authorities, who killed the two cows at a loss to the owner. No further trouble was observed among the remainder of the herd (15 in number) until the 9th of September, when four of them looked suspicious. One of them had an elevation of temperature $105\frac{2}{5}^{\circ}\text{F}$. I advised the owner to kill them on his place before the symptoms fully developed, and to watch the rest and follow the same example at the slightest indication of sickness among them; also to keep all of them in the field and not allow them to enter his barns.

CALVES FROM INFECTED DISTRICTS.

Having traveled over most of the State where the disease has existed, I would recommend that the sale of calves be restricted in the following manner: No calves should be purchased in any other State from men who make a business of buying them from stock-breeders in this State. Although they pretend to be very careful as to the stock they handle, they are often deceived, and sometimes become careless. I have been told by parties in Baltimore County, who lost stock last summer, affected with contagious pleuro-pneumonia, that they sold calves to these men, who afterwards sent them to the West. For example: In the latter part of April, Jno. A. Conklin, of Long Green, sold a calf (Alderney breed) to Louis Yoeder, a dealer in calves. It was two weeks old, of a dark fawn color, with black nose, and free from any white spots. This calf was not taken to Yoeder's farm at Unionville (this county), but was mingled with other calves from different farms which were sent to Chicago, to Pennsylvania, and elsewhere. On April 4, J. B. Manning, of Charles street avenue, acknowledged to me that a young heifer left his place that week for Fredericksburg, Va.

Regarding the stock farms in general throughout the State of Maryland, I think most of them are exempt from this disease. I have been unable to trace it except in those sections of Baltimore County which I have mentioned. There are a few men who conceal the fact until they have succeeded in disposing of all their stock, but they are few in number, and it would be unjust for us to condemn the stock-breeders in general, for many of them are men of means and in good standing. Therefore, if animals were purchased from these men with a clean bill of health, and strict attention paid to their proper removal, it would greatly lessen the danger of infection.

CATTLE QUARANTINES.

While laws have been made by the State authorities to prevent the removal of all animals from infected stables, I have failed to see them carried into effect until the outbreak which occurred on the Moore farm, in Montgomery County. Although this place was quarantined, the sick animals were not destroyed, because no provision had been made to purchase them, as the governor considered them worthless. I would advise a watch of the infected stables, especially about Baltimore; quarantine them when an outbreak occurs, and prevent the exchange of cattle which are so often made by dealers. This would prevent much of the infection of the outlying counties of the State.

Respectfully submitted.

W. H. ROSE, D. V. S.

BALTIMORE, MD., *September 15, 1882.*

CONTAGIOUS DISEASES OF DOMESTIC ANIMALS.

BY EZRA M. HUNT, M. D., Sc. D., TRENTON, NEW JERSEY.

The diseases of domestic animals are now so well and thoroughly recognized as having to do with great economical, commercial and health interests, that it is scarcely necessary to argue the need of national care in their behalf. Even ordinary diseases often mean a loss that is very large, and when epidemics occur, and devastate the herds over large areas of territory, none are so obtuse as not to recognize the damage. Our relations to foreign transportation and meat supply are now such that we cannot afford that real fears, or attempted powers started for selfish reasons, should find support in the actual existence of avoidable disease. The clear indications that are accumulating that various diseases of animals deteriorate flesh or milk, and either directly or indirectly injure the food supply, cannot be overlooked. Were it nothing else, the scarcity and high price of meats caused by epizootics, is a serious matter to the working classes. Thus, it is only too apparent that it behooves the Government carefully to consider all the practicable means for reducing or limiting the evils which thus arise, and that it provide the machinery and the funds for the enforcement of its regulations. It cannot but happen in this as in other directions that some mistakes will be made, and that inadequate plans will be put into execution. But it is only by a comprehensive view of the whole subject, and by the results of practical experience that we are able to reach proper conclusions. One of the first efforts must be in the direction of securing an oversight of the movements of animals, and especially such as are used for human food.

These movements relate to those which are imported, and to those which change hands from places near, adjacent, or from different States, and these latter for immediate slaughter, or as store cattle, or for milk purposes. As to imported cattle, it is very important that they should not bring with them any of the diseases which are common to the locality from which they come, or any other of a communicable character. It is right, therefore, that there should be proper inspection and quarantine. But are we not at present embarrassing the importation of cattle and using restrictions that are not necessary? When an animal arrives with a clear bill of health from a port where no disease exists, and in a ship which has touched at no infected port, and an inspection

shows the animal to be in sound and healthy condition, we see no reason for a prolonged quarantine. It is the old method of detention of persons as applied to animals. It has been safely relinquished in the one case and can be in the other. Indeed, whenever the same kind of inquiry is made into the condition of animals arriving as in that of men, and they are found in healthy condition and free from all disease, and the ship in proper condition, they should be dealt with as persons would be under similar circumstances. The question of quarantine should be one of judgment and not be bound down by specific limitations. During a considerable experience we have seen many cases of great hardship and unnecessary expense, such as are not being relieved by present laws. These animals, being transferred from one place to another for purpose of slaughter, if they are suspected of having any communicable disease or having been exposed thereto, the question of care has these two distinct points—to guard them on the way from the possibilities of conveying the disease to others, and to determine whether the disease as manifested or existing unfits them for meat consumption, for it is plainly determined that there are some diseases which in certain stages do not affect the meat so as to render it unsalable. Also, besides directly communicable diseases, there are others which unfit the meat for use. This gives rise to a new consideration, which is the care to be exercised over the slaughter and the sale of meat, independent of any infective character. This interest indicates the value of public abattoirs, at which all meats intended for consumption shall be slaughtered, and at which shall be present inspectors of meat who can certify its fitness for use.

The next class of store cattle—milk cows—as also of other animals not intended for slaughter, require more attention, because not only may they be already diseased but they may have the seeds of disease yet to develop, or may in some way convey it to other animals. In reference to these, the first point to be determined is, what shall be the conditions of inter-State communication? In laws and provisions as at present existing in the United States, great infelicity of administration and wastage of money are caused by the absence of all law, or the want of uniformity in law, as among the several States. It has not infrequently occurred that one State has prohibited cattle from another State on insufficient grounds, or that while there have been stringent restrictions in one State, another State with more of contagious disease has had none at all.

A full chapter in illustration of this might be written, with New York, New Jersey, and Pennsylvania as illustrations. At times these States have all had their regulative laws, yet so diverse in their methods as to make their co-operation impossible. At other times some one has been without any authority, and the interests of the others jeopardized. For a time all these were imperiled because of the insufficiency of laws in Maryland, where contagious pleuro-pneumonia prevailed. The stock-

yards of Philadelphia were for a time the distributors of contagion, not so much because they were not guarded as because of concealed infections originating from well-known southern districts. Thus each State has had to operate to great disadvantage and at an increased expense. It is highly desirable that either the Agricultural or some other national department should have power to verify this great interest and secure an oversight which shall be harmonious and exact. With such an arrangement for the past year, the efforts which have been made by these and other States would have been far more successful. All this is the more important because the exchanges of stock and milch cattle have a wide range, and because the interests of the milk as well as the meat supply are involved. The modes of limitation of the disease, and especially the epidemics of animals, are more simple than those that relate to human beings if only they are carried out with precision.

There are none of the difficulties arising from clothing or fomites or from the necessary preservation of the sick. Removal is generally easier and occlusion is defensible if necessary. It is marvellous what revelations are made to us sometimes as to the effect of change in the locality of the sick. Pens used for swine sick of hog cholera are forsaken, and the choice of an entirely new locality near by limits the disease.

Cleanliness in all its details has as essential relations to the beast as to man, and these epizootics are teaching us lessons in this regard. One of the first things to be done in an outbreak of an epizootic character is to remove all well animals from the infected spot. No matter if we do not know and cannot trace the cause of the outbreak, it is enough that it has occurred there and then, and that the experience of others has shown how often there is limitation by removal, so that the epizootic or local outbreak does not pass into an epizootic or general widespread pest. Even the sick, by removal to a similar new site, apart from the others, often recover. Beyond this we very often have illustrations of the value of individual isolation. Not only does the prompt separation of the infected animal save the others, but adds to the chance of its recovery. Facts more and more convincing show that most of these epizootics are not transmissible far in the open air. If the microbes or infective particles that convey the poison are, as is now believed, generally vegetable in their character, they belong to those species and genera which often perish in a night and are ephemeral in their type, though prolific in their production. A removal from the sphere of their operation is most hopeful. They are not near so apt to be preserved for future infliction as are the exanthemata which affect mankind.

There are some good observers who do not believe that pleuro-pneumonia is conveyed except by the breath or mouth and nose secretions of the affected animal, and that these exhalations are quite rapidly dispelled by diffusion through fresh air. At least it can be said that early

isolation promises very much in our dealing with enzootics and epizootics.

DISINFECTION.

It is important for us to realize precisely the importance that is to be attached to disinfection as applied to the diseases of animals. There is much reason to believe that the value of most methods of disinfection has been overestimated. The diluted diffusion of copperas, of chloride of lime, and of carbolic acid serves to attract attention, but are of very little practical value save where it pervades the breath or actual secretions of the animal. If either the animal or his immediate outcomes can be thus made aseptic it is of value, but we have little faith in the general sprinkling of yards and outbuildings.

The thorough use of sulphur or chlorine fumes is the only practicable method, and is advisable where the disease has prevailed. But it is to be inferred that through a proper national guard over all animal diseases by cleanliness, by prompt enforced and methodical isolation, and by disinfection and ocession if necessary, we have in hand a power of thwarting the contagious diseases of animals far more extended than that which can be exercised over human diseases. We now desire especially to consider diseases with which our duties in oversight of the contagious diseases of animals has made us especially familiar, not with a view of discussing histological, pathological, or medicinal facts, but rather as pointing to diagnosis and prophylactic management.

ANTHRAX.

This must still be regarded as the most obscure of all diseases that affect the lower animals. We certainly have come to know more of the modes of its propagation and to recognize that it is transmitted by methods once unsuspected, yet there are outbreaks so spontaneous and isolated as to admit of no well-sustained hypothesis in explanation. Such were the cases that occurred over a year since in Salem County, New Jersey, besides two or three isolated cases which have occurred in other parts of the State.

New interest has been thrown around the subject by the treatise of Darwin on the earth worm, and by the fact that now seems well established that the infective particles are often brought to the surface from the buried carcasses of animals that have died therefrom, and thus the disease again propagated. There seems good reasons for regarding the contagion of this disease as very indestructible under ground. It was my duty last year, under instructions received from your Department, to visit cases that occurred in the vicinity of Reading, Pa. I was not able at the time to secure a *post-mortem* examination of an animal, but the testimony of veterinarians and others seemed to leave no doubt that the disease as then occurring was anthrax. The cases which occurred in Salem County were very typical in their character. They were confined

to one farm, and there were losses of horses, cattle, sheep, hogs, and chickens. The owner, who had his arm immersed in some of the liquids during a *post-mortem* examination, had much swelling and inflammation of the arm, although no scratch or abrasion could be detected upon it. The cases were all sudden in appearance and in result. Most of the animals died in a few hours after any apparent disease was discovered. A bull from a neighboring herd that jumped over into the pasture and remained but half a day was dead in twenty-four hours after. Fuller details were given in the report of last year, but at that time we had not discovered in the specimen examined the special bacillus of this disease.

In the case of the death of another animal, a veterinarian from Philadelphia took some of the blood from the spleen and submitted it for examination to Professor Leidy, of Philadelphia, May 23, 1882, who reported as follows to the Academy of Natural Sciences of that city:

The specimen of the blood obtained from the spleen was examined the next day after the death of the animal. It teemed with bacteria, the peculiar form *Bacillus anthracis*, which is now viewed by most competent authorities as the cause of the frightful affection known as anthrax, or splenic fever. The *bacilli* were actually more numerous than the blood corpuscles, which appeared unchanged. The *bacilli* were completely motionless; straight, bent, or zigzag filaments, in the latter condition in pairs or mere segments. They measured from 0.006 to 0.042^{mm} in length, usually from 0.012 to 0.003^{mm}. Kept for some days in the blood, the filaments underwent division into little chains in two, three, or more dumb-bells, which measure about 0.005^{mm}, or into isolated micrococci-like particles about 0.015^{mm}. Many, however, of the filaments did not resolve themselves into these minute particles, but appeared only to grow in length and divide into segments of about 0.012^{mm} in length.

The diagnosis of these cases was thus made complete. We felt great interest in seeking out the etiology or local causation of the disease. A thorough analysis of evidence accurately taken gave no clew to any outside origin of the disease. There is great need in our American study of it that we give more definiteness both to its description and to the classification of causes.

The name anthrax has been attached to so many forms of sudden death as to make it imperative that its nomenclature be corrected, or if it has different species under one genus, that these be accurately differentiated and described. This is rendered the more necessary because *Texas cattle fever* is a disease peculiar to our country, and although often called splenic fever has never yet been accurately defined in its relation to the disease thus generally designated. This anthrax, as we saw it, is a totally different disease from Texas splenic fever. We have grave doubts whether the latter has any occasion to be called a form of splenic fever, or anthrax. The only approach at explanation that could be secured was the fact that along the low alluvial lands of the Delaware and its tributaries, near Salem, farmers had not infrequently lost horses by sudden death, and there had come to be a belief that those turned out to pasture on the bottom lands and kept there were more liable to this sudden attack.

This seems to accord with the statement of Fleming :

That it is most frequent and fatal in regions where the soil contains much organic matter in process of decomposition, and in those in which, while rich in humus, the land is retentive of moisture; in boggy countries and marshy or swampy districts, and in localities liable to frequent submersion, or in which the surface water cannot escape or is in process of slow evaporation.

We have so much of this rich alluvial land where no such disease has manifested itself that we seem compelled to look to some more specific sources of contagion.

As is the case with diphtheria and some other virulent poisons, it would seem that there are special coaptations of conditions, of ground, of air, and of individuals that disturb the natural products of legitimate decomposition and give rise to vicious combinations that act as virulent poisons. It is a great encouragement to veterinary science and art in America that the need of the closest analysis of causes is recognized, and that in the interests of agriculture this Department is now seeking with scientific accuracy for the *modus operandi* of these poisons, and, as far as possible, for their origin and nature. As our present knowledge points to the carcasses of buried animals as a source of infection, it is never safe to bury on ground that will, within two or three years, be used for pasturage. In fact the destruction of the hide, carcass, and all that appertains thereto by some alkali or by burning is almost essential.

Prof. James Law, in a report recently made in regard to an outbreak in New York State, recommends the burning of the manure as well as of the carcass. The field on which the animals have pastured should not be used until after thorough cropping. Although anthrax has not occurred to any alarming extent in the United States except as an epizootic or local outbreak within narrow limits, yet, in view of what has happened abroad, we cannot be too inquisitive as to a disease which is not confined to any one species of animals, but may in its seizures sweep off the entire live stock of a farm.

TEXAS CATTLE FEVER.

An outbreak of this fever occurred in the State of New Jersey in two distinct localities during the last year. *Post-mortems* exhibited no new facts as to it, and did not seem at all to associate it with anthrax, which had been under observation in another county the year before. It is unfortunate for the cattle interest that there is not yet an agreement among veterinarians as to the mode of contagion, much less as to its causes. The States have great occasion to ask that a series of crucial experiments shall decide whether (a) a Texas animal sick with the fever imparts the disease, (b) or whether a well Texas animal may be the host of the virulent particle, or (c) what relation animals that contract the disease from either of these bear to its propagation. Until these questions are settled by accurate tests we shall not be able to know all the modes for averting peril from a disease for which in the border States of the West the shot-gun quarantine was so long used as the protection

CONTAGIOUS PLEURO-PNEUMONIA.

The fact that pleuro-pneumonia has undoubtedly become indigenous in the United States is so potent that the pressing questions with regard to it are: In what respects does it differ from the disease as found in transatlantic countries, and how are its ravages to be abated? While the disease is essentially the same as that found in Great Britain and on the continent of Europe, it seems to have had some modification from the kind of cattle, the climate, and the surroundings. Like epidemics, it has great differences as to virulence. We have seen it so deadly as to destroy almost every animal attacked, and so mild as scarcely to affect either appetite or milk. That it is a disease from which animals recover to such a degree as not to be possible sources of contagion to any others is coincided in by most of our skilled veterinarians, who have dealt with it largely in actual practice. In most of these cases a portion of the lung structure becomes impaired and frequently necrosed and encapsulated as a foreign substance, but the general vigor of the animal is sometimes not greatly impaired.

While the contagion is the same, different outbreaks of it have their own laws of malignant or benign severity, which must to some degree be taken into consideration in dealing with the disease. This State as a whole has not been rid of the disease for several years, yet it has differed much in extent and severity, and some counties have never had a single case. By reason of its relation to great meat and milk marts, no State is so exposed to the contagion. It has often ceased here only to be introduced from some outside source. We have from time to time obtained the views of the several veterinarians who have served this Board as a guide and aid in personal investigations. The sources of contagion seem to us to be confined within narrower limits than early observation and the word of authorities seemed to indicate.

It is not uncommon for many animals to escape where they are in separate stalls, and do not inhale each other's breath or come in contact with each other's secretions. Buildings and materials do not become conveyances of the contagion in so general a way as was once supposed. If sputa are dried or remain, no doubt they may long convey the disease. But hay in the mow and other surroundings do not by mere proximity, without contact, act as fomites to any very great extent. This gives more possibility to a stamping out of the disease by isolation than was once supposed, just as is the case with many human diseases. Yet as isolation, to be effective, is a very specific and technical thing, it is not a mode of limitation to be always relied on, although it may be expedient with valuable animals to follow it out in skillful hands in place of general slaughter.

Occision, or the killing of affected animals and of the whole herd, if the disease seems extending, is still vindicated as the sovereign remedy, and is to be followed in the hands of experts who have pronounced it to be necessary.

The question of inoculation for pleuro-pneumonia comes into larger prominence the last few years. Although the plan was proposed (to some degree followed) many years since, it is not until recently that there has come to be a fuller understanding of the availability and limitations of this mode of dealing with contagious pleuro-pneumonia. The first argument that has been used against it is that unless the inoculation is done under the strictest surveillance, however effective it might be as a protection to the individual animal, the general effect would be to increase the area and prevalence of the disease. If this were true it would only result from imperfect sanitary police and from neglects not absolutely unavoidable. The question in such a case would therefore be relative, and it would need to be known which of the two were greater risks: the threatenings of the pestilence or the embarrassments attending restrictive efforts during inoculation. Another class have for a time insisted that whatever might be prudent or imprudent as to the general application of inoculation, the use of the method in herds already affected as a limitation of the spread of the contagion was not open to such a criticism. In reply to this some have denied its protecting power, while others have insisted upon occision as a more ready and efficient remedy. Two facts, however, have much modified the views of some leading veterinarians as to this matter. It is found that with proper hygienic precaution, and more precision in methods of inoculation, the mortality from the operation is reduced to 1 or 2 per cent.; also that where the tail at the point of inoculation becomes greatly inflamed, limit is put to the inflammation by its excision, and yet the value of the protection secured. Again, it is now claimed that when an animal is inoculated, the inoculated animal cannot impart the disease, and so there is not the danger of spreading the contagion which was once supposed. If this be so, it greatly modifies the view to be taken of this operation. The Board of Health of this State has long had urged upon its attention the propriety of this preventive method, but did not authorize it because the judgment of prominent veterinarians of the country were against it. Yet the fact seemed well authenticated that many owners of cattle in a part of the State had long been in the habit of thus protecting their herds, and we had never been able to point to this section as having transmitted the disease to other localities in which it occurred. As the act under which this Board has charge of the contagious diseases of animals gives it the right to order this method if it saw fit, a careful watch of all facts bearing thereon has been exercised.

Within the last year an important paper has appeared on inoculation as a preventive of pleuro-pneumonia, by R. Rutherford, M. R. C. V. S., of Edinburgh, which seems greatly to encourage the hope that some former risks are removed, and that when properly performed, we possess in it a means of limiting the prevalence of this destructive disease. His paper is contained in the June and July numbers (1882) of Fleming's

Veterinary Journal. He states his conclusions in the following summary (1-10, see page 30, Veterinary Journal, July, 1882):

1. Inoculation is based upon the theory of pleuro-pneumonia being an eruptive fever.
2. Inoculation is the application to a healthy animal of the virus of pleuro-pneumonia.
3. Inoculation does not produce pleuro-pneumonia.
4. An inoculated animal does not infect another animal.
5. An inoculated animal cannot contract pleuro-pneumonia.
6. The time occupied by the operation is from four to eight weeks.
7. Inoculation in the case of milch cows does not materially interfere with their milking.
8. Inoculated animals thrive better after the operation, and are stronger and freer from other ailments than those not inoculated.
9. The loss arising from the operation need not exceed 2 per cent.
10. From the fact that an inoculated animal is exempt from the disease, and that the average time required to develop and mature an inoculation is from fourteen to twenty-one days, that period may be accepted as the time required to arrest an outbreak.

CIRCULARS AND LAWS.

He insists upon exact methods of procuring, preserving, and inserting the lymph. His success fully justifies the provisions of our present law, while it shows the inadequacy and danger of the operation in unskillful hands. In the April Veterinary Journal of 1882, its editor, George Fleming, F. R. C. V. S., says:

By a long-continued series of experiments on animals, Dr. Willems, of Hasselt, Belgium, has succeeded in perfecting a method of protective inoculation, which is certain in its results. Further experiments with the cultivated germs of the virus are now being carried on with a view of obviating troublesome accidents which sometimes accompany the inoculation, and with every prospect of success.

The same distinguished authority, in reply to our inquiry addressed to him by this Board, says:

Inoculation, as a protective measure for bovine contagious pleuro-pneumonia, has been and is now most extensively practiced on the continent of Europe and in this country, and there is no evidence that inoculated animals, while suffering from the immediate effects of the operation, can communicate the disease. There is only one such instance recorded (it is given in my Manual of Veterinary Sanitary Science and Police), but the circumstances attending it throw great doubts upon its correctness. I, myself, discredit it. I have absolute faith in the effects of the operation as a prophylactic measure, and would most certainly counsel its adoption where the disease prevails, subject, of course, to suitable precautions as to the time and manner of performing the operation. This should be as carefully attended to as vaccination is with children.

This view, thus indorsed, justifies a change of method if it is substantiated by our experience here. Since January last we have had to deal with several herds affected with pleuro-pneumonia. Careful *post-mortem* examinations have verified the diagnosis. In one of the cases over forty students and some of the professors of the Columbia Veterinary College were present. In all about 100 cattle have been inoculated. The result has been in every instance to check the disease. The

inoculation is performed under strict oversight, and we are carefully watching results. If our American experience shall accord with that herewith given, there is every reason to hope that this threatening contagion will soon be completely under control. Without enlarging upon other contagious diseases which from time to time invade the herds and the flocks, it is important for us to know that their study is every year increasing in importance and in industrial significance. For it is by combating these that we are often caring for the health and financial interests of the people. There is great need of a unified oversight and a more liberal provision for the investigation of these various diseases. Our wide range of territory insures the care and produce of animals as a leading national industry.

The modes of conveyance, and the constant mingling of the animals from various localities, also insure the general spread of any communicable diseases.

If once *rinderpest* or *foot-and-mouth disease* should become domiciled in our land, it would involve losses in comparison with which the millions lost by Russia and France and Great Britain would be moderate. It is therefore urged that the General Government give vigor and permanency to the guarding and conducting of this great Department, and so enable us to certify to other nations that all due care for animal health and vigor is exercised, and that we can furnish to all markets the best average of meat and of milk for the use of mankind.

OUTBREAK OF DISEASE AMONG CATTLE IN VIRGINIA AND ILLINOIS.

Hon. GEO. B. LORING,
Commissioner of Agriculture :

SIR: In compliance with instructions given me at your Department October 10, 1882, I at once started for Culpeper County, Virginia, to investigate a disease reported as then existing on the farm of Mr. M. J. Wein, near Rappahannock Station. An accident to a freight train detained me at Manassas until the morning of the 11th, when I proceeded as ordered to Rappahannock Station. I then visited the farm of Mr. Wein, which is situated about 2 miles southwest of the station, and introduced myself to Mr. G. R. Coleman, a relative of Mr. Wein. Mr. Coleman lives upon one portion of the farm and has the general supervision of the entire tract, which is composed of about 900 acres, and comprises three farms. The farm upon which the cattle were kept was occupied by and in charge of a Mr. Story, and lies immediately adjoining the one occupied by Mr. Coleman. Accompanied by Mr. Coleman, I repaired to the farm of Mr. Story and obtained from him the following statement: About two months before there were nearly 100 cattle pasturing on the farm. There were several owners beside himself. The majority, however, were owned by himself and John Cunningham. About that time the first cow died, and during the two months that have elapsed twenty-one have died in all, at intervals of a day or so apart, or oftener, as the cases have appeared. Some few that were apparently sick during the time recovered, but nearly all that were affected died—the last on Friday, the 6th instant. During the time some of the stock-owners removed their cattle from the premises and took them away, and on Saturday, the 7th instant, the balance, with but two exceptions, were disposed of by Messrs. Story and Cunningham to Baltimore parties, and shipped from Brandy Station by rail direct to Baltimore stock-yards. The two exceptions were milch cows owned by Mr. Story, one of which, he says, was sick and recovered. I therefore found no other animals on the place to examine, and saw these two at a distance only. From the description given by both Mr. Story and Mr. Coleman of the symptoms of the disease, of the length of time the same lasted before it run its course, and of the appearances after death, as described by one of the farm hands, who told me that the lungs, or, as he called them, the “lights, looked like old castile soap,” I am led to the conclusion that this herd were infected with contagious pleuro-pneumonia, and that the

deaths resulted therefrom. Most of the animals that were sold and went to Baltimore were fat enough for beef, as I was informed, and in all probability they were slaughtered for that purpose, but should any of them have been reshipped and resold for stock purposes, other outbreaks of the disease may occur in the localities to which they have been taken. During the time the animals were on Mr. Wein's farm they were seen by one S. R. McClanahan, M. D., of Brandy Station, Va., a practicing physician of that place, who informed Mr. Coleman that he felt satisfied that the disease was pleuro-pneumonia.

On Mr. Coleman's farm, immediately adjoining, there were at the time of visitation 75 head of cattle, all of which had been and were, apparently, in perfect health. In answer to the question as to "whether they had purchased or pastured any cattle from the South or Southwest or any Texas cattle," a negative reply was given, and I was also informed that all of the stock that had been owned or kept upon the place was native stock and nearly all raised in the immediate vicinity or upon the farm; that, so far as known, no Southern or Texas cattle have ever been kept or driven over any of their lands.

On the 18th I received instructions to proceed at once to Sterling, Whitesides County, Illinois, and investigate the character and extent of a disease supposed to be Texas fever then existing among cattle in that county. I started forthwith, and reached Sterling, the objective point, on Friday morning, the 20th instant. I at once sought for and obtained an interview with Dr. M. R. Trumbower, a practicing veterinary surgeon of that place, who had seen most of the cases and had been in professional attendance upon some of them. The doctor informed me that most of the cases had either died or were convalescent, and that he was of the opinion I would not be able to find a suitable subject for a *post-mortem* examination. However, we at once took a horse and carriage and proceeded to the infected locality, which was situated north of the town a distance of about ten miles. On our way Dr. Trumbower gave me the following history of the outbreak: About the 1st of June Messrs. Gleason & Martin, cattle-dealers of Sterling, purchased and brought from Chicago stock-yards to Sterling about 90 head of Cherokee cattle, which were turned to pasture on their farm. On or about the 1st of September the disease made its appearance among their native cattle, and eight of them died. Immediately opposite, across the road, was a herd of native cattle running to pasture, belonging to David Wolf. Among these the disease made its appearance, and, so far as known, seven died, when the balance were disposed of and taken away, and nothing more was heard of them.

Adjoining Mr. Wolf's pasture was another, owned by John Stouffer, in which two animals died about the same time. Adjoining Mr. Wolf's pasture was one owned by Benjamin Bressler in which two animals died. Joseph Martin, another neighbor, also lost two animals. The fences between these pastures were washed out by freshets about the 1st of

July, and the cattle came in contact and run together for several days. Wm. Echternack adjoins Messrs. Martin and Gleason. His fence was washed away and 40 head of his cattle came in contact with some of the infected stock owned by the latter, which broke into Mr. Echternack's field. At the time of visitation 8 animals had been sick, 3 of which had died. Four were then sick and were apparently convalescing; one had entirely recovered. The animals on this farm had been treated by Dr. Trumbower, and the 4 seen were noted on his memoranda as numbers 1, 2, 4, and 7. I made a very careful examination of each of them, taking their pulse, respiration, &c., and confirmed Dr. Trumbower's opinion that they were in a fair way to recover. Messrs. Bressler Brothers also lost one animal, which they stated came in contact with the Cherokee cattle when they were driven along the road. D. W. Foster also lost one, but could not say it was from the same disease, as he did not positively know. John Weaver, east of Sterling, had lost 6 head, supposed to have died of the same disease.

After having had from Dr. Trumbower a full description of the symptoms, course, and peculiar characteristics of the disease, together with the lesions as he found them in the several carcasses upon which he made *post-mortem* examinations, I was entirely satisfied that the disease was Southern fever, and that it was introduced into that locality by the Cherokee cattle owned by Messrs. Gleason and Martin. This opinion was confirmed by personal visitation to the several infected farms and on hearing the owners of the infected stock describe the disease.

Mr. D. W. Foster, a special correspondent of your Department, who lost one animal, and who took a great interest in the study of the disease, gave me a very interesting statement concerning its appearance and progress, and fully corroborated the history as given by Dr. Trumbower. Mr. Foster also informed me of another singular disease that had very recently appeared upon the farm of one N. W. Ziegler, a dairyman living about 10 miles north of Sterling. He volunteered to take Dr. Trumbower and myself to the farm in order that we might see it for ourselves. His offer was gladly accepted and we drove to Mr. Ziegler's place and were received very courteously by the owner. Mr. Ziegler escorted us to his pasture, where we saw about 55 head of grade and pure-blood Shorthorns, 21 of which were suffering from an affection of the eyes, some of them in one eye only, but generally in both. Some were able to see a little, others still less; while a few were apparently totally blind. Mr. Ziegler informed me that the cattle had been running upon this same pasture all summer and had been perfectly healthy until about one month prior to that time, when he observed its first appearance in one animal. He thought it had injured its eye by some external cause. In a day or so, however, both eyes were similarly affected and gradually grew worse, until the animal appeared to be totally blind. About the same time others began to show evidences of the same affection, until 21 have thus far been afflicted. Some few

have recovered, but most of them have grown gradually worse, until total blindness has been the result.

We now drove the herd into the barn-yard and caught and examined several cases in the various stages of the disease. In the worst or well-defined cases the entire organ was the seat of extensive inflammation, the whole eye being covered with a serous deposit, cream-colored or grayish white in appearance, varying in consistency from a fluid to a semi-solid condition; and when, as in several instances, I punctured the external membranes with a sharp-pointed bistoury the contents either discharged freely, partially, or not at all, according to the consistency of the deposit. In all of those where the deposit was fluid the animal could see quite well. In those where the deposit was thicker the vision was less perfect, while in those in which it was semi-solid the animal appeared totally blind. The membrana nictitans was fully injected and the eyelids swollen and discolored, and all parts of the organ and its coverings were very much inflamed.

Having satisfied myself with the examination of these very interesting cases, I turned my attention again to the pasture to see if I could find any local cause for the peculiar affection. The pasture is composed of blue grass and timothy, is bottom land with a stream of clear water running the entire length of it. Although the land is wet and marshy in very rainy seasons it was at this time perfectly dry and solid, and Mr. Ziegler stated that he had used it for the same purpose for a number of years and had never had any difficulty with his stock before. I could not therefore find any positive cause for the appearance of the disease at this time. I think it ophthalmia of a severe form, and probably due to local, malarial, or perhaps to constitutional causes. If from the latter I cannot see why it should affect so many at the same time.

In answer to the question concerning the general health of other domestic animals, Dr. Trumbower informed me that so far as he knew they were generally healthy. There had been a few cases of the prevailing epizootic, pink-eye, among horses, but it had not become serious as yet. He informed me also that he heard that there had been a disease among cattle some time before in McLean County in that State. I therefore resolved to visit the county seat of said county, and obtain for your Department all the information I possibly could concerning the rumor. Therefore, on Saturday morning I took an early train via Illinois Central Railway to Bloomington. On my arrival I took measures to obtain the desired information, and was referred to Dr. Williams, veterinary surgeon, upon whom I at once called. I was unfortunate in not finding him at home, but through the kindness of Mr. Wm. H. Flack, a stock-dealer, I was introduced to several stock-raisers and cattle-dealers, and from them I obtained some very reliable information. One gentleman informed me that during the early summer or late spring months he had seen some cattle (fat steers) that were afflicted with sore eyes (and from his description I think it the same as that now affecting

Mr. Ziegler's herd in Whitesides County). He further stated that some of them became totally blind, while others were affected in one eye only; that most of them recovered, and that, although it caused a little excitement in the immediate neighborhood for a short time, it soon died away, and the cattle got fat and were sold without any loss to the owner as far as he knew. I suppose these to be the same cases of which Dr. Trumbower had heard. I next visited Pana, Christian County, 90 miles further south, and solicited information from that section of the State. Mr. Allen, a large farmer and stock-grower, informed me that, as far as he knew, animals of all kinds were in a healthy condition. There had been no losses other than the natural losses incident to stock of all kinds since last winter and spring, when very many cattle and swine starved to death owing to the failure of crops in that part of the State, and the complete absence of feed during the latter months of winter and the early spring. Many of the farmers were not in possession of sufficient means to purchase feed, and suffered severe losses in stock in consequence.

On Monday, the 23d, I started eastward via. Indianapolis, Terre Haute and Saint Louis Railroad. Stopping at Charleston, the county seat of Coles County, I called upon and introduced myself to Isaac J. Miller, veterinary surgeon of that city, who not only has a large professional practice, but is largely interested in raising and fattening cattle as well. In answer to my inquiries concerning the general health of domestic animals, he stated that there was considerable epizootic pink-eye among horses, and that there had been some splenic fever in some sections, which, in most instances, was introduced by cattle from Missouri or the Southwestern States, having been brought into this section of Illinois from Saint Louis stock-yards, or direct from the South. Some few stock-growers purchased Texas cattle, but most of the large farmers were afraid to buy them, during the summer months especially. Having thus received information from various sections of the State, and from localities in which are situated some of the largest stock-growers of Illinois, I resumed my journey homeward, and arrived in Philadelphia on the afternoon of the 26th inst.

I have the honor to be, very respectfully, your obedient servant,

WM. B. E. MILLER, D. V. S.

CAMDEN, N. J., *October 31, 1882.*

DISEASE AMONG CATTLE IN ILLINOIS.

In the latter part of February last, Dr. H. J. Detmers, of the veterinary corps of this Department, was directed to examine a herd of cattle belonging to Mr. E. Linder, residing near Mattoon, Ill. A disease unknown to the veterinarians of that locality had prevailed in the herd for some time, and as it had proved quite fatal there was an earnest desire among the stock-raisers of the vicinity to have its nature definitely determined. The following detailed report from Dr. Detmers gives a history of the herd and the character of the malady:

Mr. E. Linder owns a large farm of 1,300 acres some 3 miles southwest of Mattoon, Coles County, Illinois. His herd of cattle is at present composed of 108 head; usually at other seasons it numbers between 150 and 200 animals. It ought to be stated that the herd is not entirely of his own raising; that every winter and spring, this year excepted, quite a number of young cattle are bought, mostly in the southern part of Illinois and in Indiana, where they can generally be obtained much cheaper than in Central and Northern Illinois. About 30 animals, mostly yearlings, have died since the fall, 1881. Mr. Linder was somewhat frightened, as he feared the disease might be contagious, and he did not purchase any young stock this winter. As his fat steers were sold and shipped in the fall, the unusually small size of his herd at this season of the year is thus accounted for. About two years ago he bought a yearling Shorthorn bull in Indiana. It appeared to be a fine animal, but soon commenced to show symptoms of sickness, or of being affected with some lung trouble. He never shed his coat of hair in a proper manner, and is at present, though in a very good condition as to flesh, a small, narrow-chested, stunted, or runty-looking animal. In the fall of 1881 most of the yearlings of Mr. Linder's own raising commenced to show symptoms of sickness, and about 40 per cent. of those that were taken sick died, some of them in the fall, and some in the winter or toward spring; and in the fall of 1882 the disease again made its appearance among the yearlings, and 12 or 13 more died. The whole loss since the fall of 1881 amounts to about 30 or 31 animals; this number, however, includes a few old cows, which died in the spring of 1882, and, as Mr. I. V. Linder, a son of Mr. E. Linder, thinks, from other causes.

On the morning of the 3d instant four yearling heifers were pointed out to me as being affected with the disease that had proved fatal to the other animals. They were in poor flesh, very thin, more or less arching their backs, conspicuously moving their flanks at each respiration, and had profuse diarrhea. When catching the worst-looking and evidently the sickest one in the lot, an animal which showed considerable weakness in the hindquarters, not yet perceptible in the others, and had the most profuse diarrhea, I found its temperature, taken in the vagina, to be as low as 97°.7 F. All visible mucuous membranes looked pale; those of the nose somewhat dirty; the respiration, though not perceptibly accelerated, was executed with very conspicuous flank movements. Auscultation and percussion revealed some density—impermeableness to air—in the posterior portion of the left lung, and some fluid (serum) in the lower part of the chest, but nothing else abnormal. As the animal was evidently in a sinking condition, or past recovery, and was expected to die in a few days, and as no recent death

had occurred, it was concluded to kill it by bleeding for a *post-mortem* examination. The blood was very deficient in quantity, rather thin and dark-colored—both carotides and jugular veins had been cut—and coagulated readily to a uniform mass. On opening the chest the lungs, rather poorly collapsed, presented a speckled appearance; some lobules were found to be at the point of hepatization; some contained yet fluid exudation, and some were emphysematic, while most of them presented a normal appearance. Exudation and incipient hepatization, although present in some lobules in every portion of the lungs, were most extensive in the posterior portion of the left lung. The interlobular connective tissue, however, was comparatively free from any exudation. On opening the bronchial tubes by means of a pointed pair of scissors, the finer ramifications, particularly in the posterior portion of the lungs, but also in the anterior lobes, were found to contain a large number of worms (*Strongylus micrurus*), and indications—swelling of mucous membrane and abundant exudation in the adjoining tissue—that many more had been present, but probably had been expelled by sneezing and coughing. The pleura was everywhere perfectly smooth, and the amount of serum contained in the chest was not very large. Its quantity could not be correctly estimated, because by cutting the œsophagus and the vena cava it became mixed with fluid from the stomach and with blood. In the abdominal cavity, which contained much more serum than the chest, I estimated it at about half a gallon or more; the liver, spleen, kidneys, urine, bladder, and the paunch, second and third stomach, and their contents, were found to be normal, or not presenting any morbid changes. The contents of the fourth stomach (abomasum), however, were perfectly liquid; had the appearance of very dirty water containing some vegetable rests, and its mucous membrane was very much swollen or loosened and inflamed, and presented on its surface a plainly developed papular appearance. In the small and large intestines, too, the mucous membrane, though in a less degree than in the fourth stomach, was much swollen and somewhat inflamed, or in a catarrhal condition. With the exception of considerable enlargement in nearly all lymphatic glands, but particularly in those of the mediastinum and mesenterium, no other morbid changes were found.

As the *post-mortem* examination fully explained the nature of the disease, and as the animals that had died had presented the same symptoms, it was not deemed necessary to kill any more of the yearling heifers.

As the entozoa brood of *Strongylus micrurus* is deposited, mostly at least, between September and the middle of May, passes its embryonal existence in low, wet places and small water holes, I advised Mr. Linder to keep his healthy calves and yearlings, and particularly the calves to be born in the spring, on dry land, destitute of low, wet places and water-holes, and away from those pastures which are or have been occupied by the diseased animals between September and May. If he complies with this advice I have no doubt the disease will soon disappear from his farm, unless new worm-brood is introduced with animals raised in infected districts. Whether the worm-brood was originally introduced by the Shorthorn bull from Indiana, which I have no doubt suffered from lung-worms, or whether the infection came from other cattle bought in Southern Illinois or Indiana, I cannot decide. Mr. Linder, jr., is inclined to think it was the bull that infected the pasture. Previous to the fall of 1881, Mr. Linder's pastures were considered to be healthy ground; at least no uncommon mortality prevailed among his cattle. His farm is on slightly rolling ground, and with the exception of a small portion, remarkably free from low, wet places and stagnant water-holes.

CHARBON AMONG CATTLE IN MISSOURI.

The following letters were received by the Department in January last :

HOUSE OF REPRESENTATIVES,
Washington, D. C., January 19, 1883.

Hon. GEORGE B. LORING,
Commissioner of Agriculture :

SIR : I inclose you herewith a brief communication from a large cattle dealer in Audrain County, Missouri, to which I desire to call your special attention. It is in the midst of one of the largest cattle-feeding districts of the West, and from a dispatch received last night, I learn that the disease is attracting great interest and alarm in that section. If you have authority to do so, I beg to suggest that you send an expert to examine into the facts, and, if possible, suggest measures to prevent the spread of the disease.

I have the honor to be, your obedient servant,

A. H. BUCKNER.

The following is the inclosure alluded to by the Hon. A. H. Buckner :

SANTA FÉ, MO., *January 15, 1883.*

SIR : I have a herd of cattle, about 100 in number, that are suffering with a disease pronounced by Dr. J. S. Drake and others to be pleuro-pneumonia or rinderpest. Several have died and the remainder are nearly all sick. If there is any department of the Government that takes cognizance of disease among animals, you will oblige me by informing it of this outbreak among my cattle, that I may be advised of what is best to do.

Very respectfully,

J. M. McFADIN.

Hon. A. H. BUCKNER,
Washington, D. C.

On the same day another letter reached the Department from Mr. Buckner, in which he urged a speedy examination for the purpose of determining the nature of this disease, and inclosed the following additional letter from the locality of the outbreak :

MEXICO, MO., *January 19, 1883.*

SIR : The people are very much alarmed over the McFadin cattle, as they think the disease contagious. It is very fatal, and as a large proportion of the wealth of this section of the country, as you know, is in this class of property, I hope you will do everything in your power at once to arrest the progress of the disease and quiet the fears of the people.

Yours, truly,

H. A. RICKETTS.

Hon. A. H. BUCKNER,
Washington, D. C.

The Department directed Peter Harding, D. V. S., of Dodge City, Kans., to at once proceed to the locality of the outbreak, determine the character of the malady, and give the necessary advice for its speedy suppression. His report, which was promptly rendered, was as follows:

Hon. GEO. B. LORING,

Commissioner of Agriculture:

SIR: I received your orders on January 30 last, directing me to proceed to Audrain County, Missouri, to investigate and determine, if possible, the cause of a disease then prevailing among cattle on the farm of Mr. J. M. McFadin, located near the village of Santa Fé. On my arrival I proceeded to the place indicated, and after a thorough investigation I found the animals suffering with a disease known as charbon or anthrax—a blood-poisoning disease, which was caused by feeding on spoiled or tainted food, and from running on a low, swampy pasture filled with stagnant pools of water. I separated the animals and changed their diet, and had them placed in a good locality, in a sanitary point of view. Since then there has been no increase of sickness, nor have any more animals died. They are all now improving very rapidly. The herd consisted of 100 head, out of which 50 were affected and 9 died. The animals were not imported, but were raised in the neighborhood. A disinfectant has been plentifully used wherever the animals have been pastured or confined. The disease has not appeared elsewhere in the county, and the people now feel secure against its further spread.

Respectfully submitted.

PETER HARDING, V. S.

MEXICO, AUDRAIN COUNTY, MISSOURI, *February 6, 1883.*

NOTES FROM CORRESPONDENTS.

CALVES AFFECTED WITH *STRONGYLUS MICRURUS*.—In March last a letter was received from Mr. J. B. Varnum, Albia, Iowa, giving the symptoms of a lingering, but fatal, disease, which was then prevailing among his calves, and asking advice as to their treatment, &c. He was informed that his animals were suffering from the effects of a worm known as *Strongylus micrurus*, and advised, as a remedy for this lung parasite, to confine the afflicted calves in a close building where they could be subjected to the fumes of burning flowers of sulphur. To prevent the air from becoming too highly charged with the fumes of the sulphur, and thereby causing violent coughing, the attendant should remain with the animals during the fumigation. The application should be continued for half an hour at a time, and should be repeated several days in succession, and at intervals of a week for several weeks, so as to destroy the young worms as they are hatched out in successive broods. Until all cough and excitement of breathing have passed the afflicted animals should not be considered safe to mix with healthy ones, or allowed to run on a non-infected pasture. A herd of cattle in Illinois, afflicted with this disease, was recently examined by Dr. Detmers, whose report will be found elsewhere in this volume. As the symptoms of the disease as they appeared in Mr. Varnum's herd of calves may prove of interest and value to others, his letter is herewith inserted :

Permit me to call your attention to a disease of stock which is new to me. My calves, when three or four months old, commence to cough. Although they retain a good appetite they lose flesh rapidly, and the most nourishing food, which they eat voraciously, fails to supply the waste of disease. Their symptoms are precisely those of a human being suffering and dying with a lingering consumption. After a few weeks, though treated with the best of care, they die of pure exhaustion. A *post-mortem* examination reveals nothing to indicate disease except the lungs. These are in a high state of inflammation and filled with clots of blood. By opening the bronchial tubes I find them filled with myriads of small worms perfectly packed into every orifice. These worms are about the size of and resemble bits of eight-spool cotton about 2 inches long. Placed under a magnifying glass they have the exact appearance of the worms found in the human stomach. They are translucent. I have never found them adhering to the membrane or in the tissues of the lungs, and conclude they do not subsist on the solids, but exhaust the fluids. They are very lively when the warm lungs are first opened, but die immediately when exposed to the air. They are tender and cannot be preserved to send to the Department. In fact, a *post mortem* must take place at once if anything is found except a purple putrid mass. I have no knowledge of this disease outside of my own herd, and cannot say whether it is confined exclusively to calves or not. Many of my cows have had an unusual cough for the past year, but none of them have died. I examined five of my calves that died, and found the lesions similar in every case.

FATAL DISEASE AMONG SHEEP.—Mr. A. Bennett, Belle Meade, Kans., writes as follows concerning a fatal disease among sheep in that locality :

A very singular and fatal disease has recently made its appearance among the sheep in this section which is causing great loss, decimating flocks at a fearful rate. I am not able to fully describe it, not having the time to give it that investigation necessary to detail its course minutely. In general terms, the animals seem to lose flesh, weaken and die, with no apparent pain or sickness. It seems to attack the lambs first, and some large herds here lost all of the lambs. And yet it misses some flocks entirely, visiting others on either side and nearly wiping them out. This, too, when all flocks are cared for about the same way. The oldest sheepmen here never saw anything like it; neither can they account for it.

As the sheep industry is of great importance in this vicinity I thought it best to make the general fact known to the Department. I presume by taking a little time and visiting all of the ranches, and investigating water, ranges, corrals, &c., and dissecting a few sheep, some information could or would be gleaned that would throw some light on the matter. This would entail some expense, but I think you will agree with me that this is an important matter. I think the losses in this immediate vicinity have been fully 5,000 head since November 1, most of the losses occurring since January 1.

CATTLE INJURED BY EATING CLOVER.—Mr. Henry C. Moseley, of Carpenterville, Kane County, Illinois, writing under date of July 5, 1882, gives the following account of injury to cattle caused by eating young and succulent clover :

Inasmuch as the cattle in this section have been injured and killed by eating clover, I will narrate a few facts concerning the matter.

The farmers of Carroll and Ogle Counties have a very large acreage of clover, which was sown for fertilizing purposes, to prevent the land, where it was somewhat rolling, from washing, and also for pasturage. Soon after the cattle were permitted to graze in the fields (the past spring) the weather became very cool, and remained that way so long that the grass grew very slow, and was, in fact, so short that very few pastures afforded an adequate supply of feed, and the cattle, therefore, got reduced in flesh; and when the warm weather came, and there were frequent copious showers of rain, the clover grew very rapidly, and when young, tender, and wet, so much gas was engendered that the stomachs of some of the cattle were greatly distended, and in the vicinity of Lanark, Shannon, Baileyville, Maryland, Foreston, Mount Morris, Harper, Holdane, Polo, and Brookville there were over 100 animals during the past six weeks that have suffered from its effects, and it proved fatal to 45 head of cattle.

The farmers have resorted to different expedients to relieve their afflicted animals. The most popular remedy is to place a stick besmeared with tar in the sick animal's mouth, and then attach it (the stick) by a cord to the horns, when the animal, in its fruitless efforts to lick the tar or remove the stick from its mouth, will soon begin to belch, and, finally, it obtains gradual relief. I. Sleiffer states that a quantity of black pepper placed on the animal's tongue will cause it to cough and sneeze, and he considers that an effectual remedy. H. Williams asserts that he once knew nine animals that were badly bloated to be relieved by putting a tablespoonful of soda in a pint of warm lard and administering it to them. John Anderson's antidote is salaratus, dissolved, and a heavy dose administered. He avers that the stock in this region were similarly affected a few years ago, and that in a very wet season it is more liable to be attended with fatal results. A few stockmen still adhere to the ancient and barbarous methods of "stabbing them in the back." I recently observed two valuable cows which had been "stabbed" because their stomachs were distended; one of them was apparently well, while the other is seriously, if not fatally, injured.

Where cattle have had access to blue grass, or have grazed in fields containing both clover and timothy, they have sustained no injury. The dairymen and stockmen now concur in the opinion that it is impolitic to sow clover exclusively for pasturage. They state that they would prefer, when sown for that purpose, to have one-third of the seed timothy and two-thirds clover.

SUPPOSED CURE FOR FOWL CHOLERA.—Mr. James B. McCrellis, correspondent of the Department at Vevay, Ind., writes as follows, under date of February 3, 1883:

The circular issued by your Department of date February 23, 1881, giving the results of experiments made by D. E. Salmon, D. V. M., for the prevention of what is commonly known as "chicken cholera," was given as wide a circulation by your correspondent as possible. A cure for the disease has, I believe, been discovered by Mrs. Viola Dufour, of Jefferson Township, in Switzerland County, she residing about one mile above Vevay. Mr. Francis R. Dufour, her husband, carries on farming and raising poultry quite extensively, and has been quite a loser by cholera. Mrs. Dufour experimented with a weak solution of carbolic acid mixed with the corn meal used as food with surprising results. Chickens that gave evidence of being ready to die were fully restored by eating food so prepared, and when they were not able to eat it was fed to them by forcing it down their throats. The cholera disappeared, and while it has been raging in the neighborhood, Mrs. Dufour's poultry have escaped. The above is the result of repeated experiments made by Mrs. D., and it seems to be an effective cure. I would recommend sprinkling a weak solution on their feeding ground, or where it is customary to feed them.

CONCERNING CONTAGION.—The following letter of Mr. Wolff, concerning the origin and transmission of contagious diseases, will be found interesting:

U. S. SENATE CHAMBER,
Washington, February 7, 1882.

HON. GEO. B. LORING,
Commissioner of Agriculture:

SIR: In compliance with the request of the writer, a constituent of mine and veteran of the last two wars, Capt. Francis A. Wolff, I inclose the following communication, to which I respectfully ask your attention. The theories he advances are at least curious, if not suggestive.

Yours, truly,

L. Q. C. LAMAR.

WALNUT STATION,
Tippah County, Mississippi, January 9, 1882.

HON. GEO. B. LORING,
Commissioner of Agriculture:

SIR: I have just read with great pleasure and a lively interest the highly valuable report on "Contagious Diseases of Swine and other Domestic Animals, 1880." Dr. H. J. Detmers opportunely asks, "Where do the swine plague schizomycetes come from?" With your indulgence, I shall happily avail myself of the opportunity of a reply.

The Chinese sugar-cane was introduced into this country from France in the year 1856. Almost simultaneously hogs were stricken down with cholera, and oats and blackberries ravaged by the rust.

Bacilli in swine and the organisms that make up the rust derive their parentage from the myriads of parasites that sip the nectar of cane. From the lesions of microscopic organisms that characterize rinderpest it would not be too great a stretch of the imagination to derive from the same source. This view is highly corroborated by Reynal, who points out that it "proves philologically the region in which the disease originates, or, rather, permanently reigns, is in the far East." It is said that cases of

rinderpest in the United States are chronicled for many years past, which I think must be taken with some grains of allowance, as its malignancy was never noticeable until after 1856. These diseases are no longer an "exotic." The presence of sorghum makes them *indigenous*.

Pleuro-pneumonia is a concomitant of our advanced civilization. It originates in stampedes and the perturbations of rail and water transportations, the change of locality, the sundering of ties, homesickness, and bereavements.

The failure to cure diseases by remedies that were considered hitherto efficacious, and by some a specific, can best be explained by some observations of mine on measles. During the Mexican war the First Mississippi Rifles, commanded by Col. Jefferson Davis, camped in due form at the mouth of the Rio Grande. While one company rested on the shelly shore, Company I, of which I was a member, occupied a portion on the extreme north, and on the leeward. Near by, however, was a huge coal-heap and some new military store-houses. When the measles struck in on Company I, only Colonel Davis manifested great uneasiness, as he was then figuring with all the skill of a diplomat to secure a position in the front, that he might share in the renown that awaited his illustrious father-in-law. Besides, there were many of us like "Norvell on the Grampian Hills," who had "often heard of wars and longed to follow them like a warlike lord."

But to get back to the company. We were magnetically and positively charged with all the contagion and impurities of the regiment. Had there been an eleventh company beyond us, we would have enjoyed an immunity. Probably the same result would have held good on a pen of swine or a kennel of dogs. Our company was soon cut loose from the regiment and quarantined a mile up the river. No new cases appeared, and we were soon on the march in the path of glory.

During the war between the States, in rendezvous south of Grenada, Captain Pitman's company from Choctaw County camped near the west side of the railroad, and my company from Tippah County adjoining, and about 100 yards farther west and on the windward—for the wind blew mostly from the west. The measles went all through Pitman's company. Although the men in both companies mixed and walked in groups to and from Grenada, my company escaped the infection. Probably had we remained there a whole year under a west wind all the current diseases would have preyed upon Pitman's company, while my men would have waxed in health and vitality. A few months later, however, while I was in command of a detachment of five companies, quartered in a cotton-press shed in New Orleans, all the susceptibles took the measles—and some other diseases.

In summing up these results I have but one doubt in regard to the potential agency of wind. In the first place, Company I was contiguous to a little commercial center; and in the latter case Pitman's company was next to a railroad. These phenomena could be well understood if some veterans will recall to mind and publish their observations on wind, coal-heaps, and peculiar localities connected with measles in camp.

While some of these agencies are irresistible in the inception of diseases, I think we may safely conclude that as the human family cook victuals to make them more palatable and more digestible, and to prevent the ravages of trichinæ and many animalcules of that other world, why should we hesitate to boil some food given to animals to prevent and cure the spread of disease?

REMEDY FOR SWINE PLAGUE.—Mr. J. E. McCalm, Vermillion, Marshall County, Kansas, gives the following remedy for swine plague:

Finding a herd of hogs affected with swine plague, I determined to experiment with the disease. The hogs were in good condition, and weighed about 300 pounds each. I secured some podophyllin and leptandra, equal parts, and mixed. Of this preparation I administered three grains as a dose, and the first day gave three doses. On the second day I gave two doses and on the third one. The medicine acted both

as an emetic and a cathartic, and to my astonishment all the afflicted animals (six in number) treated in this way recovered

BLACK LEG.—Outbreaks of black leg were reported in several localities during the past year. Mr. Ernest Eggerth, Communia, Clayton County, Kansas, gives the following account of an outbreak of the disease among both cattle and hogs in that locality:

We have had a disease here among our cattle and hogs similar in its symptoms and effects to black leg, though it seriously affects the throat. A *post-mortem* examination shows the windpipe much inflamed and lined with coagulated blood. The stomach seems to be in a healthy condition, though occasionally the intestines show inflammatory signs. Under the skin, on the shoulder, or other parts of the body, there are lumps which, on pressure, emit a rattling sound. The hogs were generally afflicted with a swelled neck, had great difficulty in breathing, and generally died within twenty-four hours.

Mr. E. A. Galbraith, Doughertysville, Hawkins County, Tennessee, says that a great many young cattle have died of this disease in that locality. It proved fatal in every instance.

Mr. E. L. Gary, Alamota, Lane County, Kansas, says:

I have lost several one and two year old calves during the past season by what is supposed to be black leg. Two of these animals were fine Holstines, two years old, which I brought from the East this spring. I have lost seven head in all. I reside only 2 miles from the Texas cattle trail, and some have claimed that the disease of which they died was Texas or splenic fever. The animals lost the use of their hind quarters, which seemed to be the first symptom. They died in from two to four days after the appearance of the first symptom. I opened one yearling bull, and all the organs seemed to me to be in a healthy condition, except the stomach, which was full of well-masticated food, but which was very dry and hard, and I doubt if it could ever have been voided. Some of my neighbors pronounced the disease "dry murrain."

Mr. William H. Harman, Little River, Floyd County, Virginia, writes as follows:

Permit me to call your attention to a disease that is proving very fatal to my cattle. The disease seems so far to be confined to calves and yearlings, and is known here as black leg. The first symptom with some is a slight running at the nose and an occasional cough. Others become suddenly lame, generally in one leg, oftener in the hind leg, but occasionally in the fore-leg, and these do not have the running at the nose. After the lameness has continued for five or six hours, the afflicted limb becomes completely paralyzed. They generally died within twelve hours after showing the first symptoms. On skinning the afflicted part, immediately under the skin is found a jelly of black clotted blood, which extends in about the same condition to the bone. When the disease is located on the hips, which is generally the case, it seems to penetrate to the kidneys, and from the kidneys along the blood veins to the heart. I think the trouble is caused by a stoppage of the circulation of the blood; that mortification sets in where the blood does not circulate; that this mortification follows up the veins and arteries until it strikes the heart, when death ensues.

BRONCHITIS IN CATTLE.—Mr. Willett Stedman, Lee Centre, Oneida County, New York, says:

There have been three cases here, two of which have proved fatal, of a new disease among cattle, but as the cases occurred in as many different herds and the disease did not extend to other cattle, we have called it "bronchitis." The symptoms were as follows: The animal had great difficulty in breathing, would pant at intervals as the

disease progressed; the pulse would also increase from 65 to 75. The symptoms were accompanied with pain, as the animal would frequently groan. Food was refused, the bowels would become inactive, and within a week from the time the first evidences of illness were observed the animal would die. A *post-mortem* examination of the two that died showed the lungs to be filled with clotted blood; the lungs were also abnormally large and solid.

ANTHRAX.—Mr. J. H. Bullock, Los Angeles, Cal., writes that a very destructive disease has prevailed for some time past among cattle in that locality. Mr. Bullock says:

In this section of the State we are being now troubled (much to our cost) with a new disease among our cattle, which has so far baffled all our local doctors. It made its appearance the present summer, and we have lost nearly every animal that has been attacked. It seems to be a "murrain" and is called here by a local name, viz., "big melt," the melt of the animal being very much enlarged, as well also (in three of my cows that have died) as the liver, lungs, and kidneys. In one cow one of the kidneys was natural, the other twice the natural size. The animal appears "dumpish," slobbers at the mouth, and the nostrils discharge, but not of any offensive odor. My neighbor cured one heifer with a very large dose of common salt, which, however, did not save mine. I gave Glauber salts and saltpeter, but they all died. They only live about twenty-four hours. They appear also to suffer with a difficulty in the throat, rendering breathing laborious and difficult. In all cases the lungs are very much swollen and inflamed. I cannot give a technical diagnosis, of course, but it appears to be a dead shot.

EPIZOOTIC OPHTHALMIA.—Several instances of epizootic ophthalmia occurred among cattle in widely separated localities during the past season. Mr. J. M. Graham, Rinewood, Tenn., writes as follows concerning an outbreak of the malady in his neighborhood:

A disease new to our section has broken out among the cattle, and we write to you for information concerning it. It first appeared in the calves, and as far as we have observed and have been informed, it attacks and works entirely upon the ball of the eye. The first symptoms are weakness of the eyes, accompanied by running water or weeping, no matter being discharged. The eye continues to inflame, and it finally goes entirely out, assuming a pink or red appearance about the pupil. The animals otherwise appear to be in good health and fine condition. None have so far died, but a number have lost one or both eyes.

Mr. A. H. Weaver, Owl Run, Fauquier County, Virginia, writes as follows, under date of April last:

I have eight milch cows that have been suddenly stricken with blindness. The left eye in every instance was the first to become affected. After a few days the right eye would show the same condition, resulting in total blindness. I have called in several of my neighbors, and we have all come to the conclusion that it is a new disease with us—that none of us have ever seen or heard of a case of it before.

Mr. J. B. Twill, Burlington, Coffey County, Kansas, says:

During the month of August my attention was called to a peculiar form of disease which had made its appearance among the cattle in the vicinity of what is known as Otter Creek, this county. When first noticed the eyes were "running water" continually, showing a high degree of inflammation. In a few days the sclerotic coat began to assume a whitish color and grew worse, till in many cases the animal was blind. At this stage the eye had the appearance of the white of an egg and the whole eye appeared to be slightly enlarged. The animal after a time usually recovers its sight. In some cases but one eye is attacked, in others both. The farmers have no

idea of the cause. It cannot be the dust through which those cattle pass in going to and returning from water, as calves that have been confined have suffered also. Perhaps the extreme drought and excessive heat may have something to do with it.

SUPPOSED RABIES IN A COW.—Mr. Jerome Wiltse, of Falls City, Neb., gives the following account of the death of a cow from supposed rabies:

We recently lost a cow under peculiar circumstances, and are in doubt whether it was a case of rabies or one of Southern fever. Many dogs have run mad in this vicinity during the last six or eight months, and some cattle have been bitten and died of the disease. In all known cases, I believe, when in a delirious state, they have attacked men and beasts, when not restrained from so doing. Several cattle have died with symptoms somewhat similar, though not inclined to hook people. The one we lost did not appear to be sick until the evening of the 20th instant, when she was restless and continually on the move. Though she had eaten salt and grass and drank water that afternoon, she looked gaunt and her general appearance was that of an animal that had been sick some time. She did not show any violence, but appeared desirous to remain near the other cattle.

On the morning of the 21st she attacked three of the other cattle, but was too feeble to do harm. This she soon abandoned and went to walking around a hay-rack, the muscles of her neck and flank twitching, and frothing at the mouth. She would at times turn around to the left, as if trying to get her nose to her tail, or as a dog does before lying down. Her eyes had a glassy look, and her excrement she seemed unable to discharge. When approached in front she would give her head a slight toss up as cattle do when not wishing people too near them, but could be readily driven wherever we wished her to go.

At 7 o'clock in the morning she was given water. She took a mouthful, and, bawling, staggered and nearly fell forward. This is the only noise she made except a continuous groaning, which she had kept up from early in the morning. At 8 o'clock I offered her water; she went to the pail and, without trying to drink, placed the front of her head against the side of it and held it there several minutes, appearing to rest it. At 10 o'clock she was seen resting her neck and head on a cultivator. At 11 o'clock the boys found her down and dying. She died with scarcely a struggle, several quarts of frothy water running from her mouth. Her appearance had been such as not to attract our attention until the evening before—about eighteen hours. We have had dry weather since the 27th of July last, and the thermometer has frequently indicated 100° above zero in the shade, and under the circumstances we are at a loss to determine the cause of the disease. The cow has not been exposed to any diseased cattle, nor do we know of any rabid dog having bit her. Our own dog, now living and well, bit her some six weeks ago. A great-granddam of hers had brain disease, but recovered after a winter's sickness.

The following singular experience in the prevention of rabies is given by Mr. Leopold Trakat, of Saginaw City, Mich., which will no doubt prove interesting in connection with the above case:

About forty-five years ago a great number of cattle in a herd belonging to a large estate in Eastern Prussia had been bitten by the herdsman's dog. The man did not suspect his dog to be mad, as the "usual signs" were wanting; but when at last his boy was bitten, and shortly after he himself received a severe wound, he began to be alarmed. Leaving the pasture in great excitement, he hurried to the inspector of the estate, telling him what had happened. The bad news spread quickly over the whole community, and soon a heartrending clamor filled the air, for most of the injured animals belonged to the poor working people. Everybody seemed at a loss what to do in this emergency. Then one of the bystanders had some recollection of a certain

man, in a near village, as having once performed a cure in a similar case, and he urged them to send for him immediately, which was readily agreed to. The man arrived in a short time and commenced business at once. Every animal that was actually bitten or suspected of having received a slight wound was subjected to bleeding. When about half a pint of blood had been obtained, it was mixed with nearly the same quantity of good strong vinegar, and by means of a bottle was poured down the gullet of the animal. Then a small slit was cut into the skin, over the shoulder blade, and a very thin slice of a certain root, which the "doctor" took from his pocket, was put into the slit and shoved below the skin. Then the bottle and surgical instruments were scrupulously cleaned, and the same operation was repeated with all the other animals—about 15 or 20. There were left, however, two large oxen, belonging to the proprietress of the estate, a well-educated and enlightened (or strong minded) lady. When the inspector went over to ask her permission of having performed the treatment (as the cattle were surely bitten), she strongly objected to what she called "nonsensical hocus pocus," and ordered her oxen to be brought into a separate stable and well taken care of. But in a few days all the fearful signs of hydrophobia set in, and the two oxen had to be killed, while all the other cattle were permanently cured.

I have not been able to learn with certainty how the two persons escaped their fearful fate. Somebody told me they had eaten the wasted heart and liver of the mad dog, the killing of which, of course, having been the very first transaction of the excited people; but as they cared more for their cattle than for their cattle's pastors, I could not get any satisfactory information. Respecting the root used by the "doctor," I found out afterwards that it was of a plant belonging to the hellebore family. It is white, and when cut transversally shows a black, crooked mark in the middle, resembling the letter C. The Germans call it, in that locality, "Christwurz." But I believe it forms no part at all in the cure of rabies, and is certainly a very superfluous addition. This will clearly appear from the following narration: A short time before the great rebellion broke out, I had in my evening school a young man who was troubled with epilepsy. He had contracted the malady by swallowing, on a wager, a whole bottle of whisky at a draught. All the nostrums which he tried—and there are myriads afloat—proved worthless in his case. When the war actually broke out he had a great desire to enlist, but did not dare on account of his sickness, fearing a speedy discharge. He was completely cast down. Then I told him how the cattle were cured, and pointed out to him the similarity of the symptoms in epilepsy and hydrophobia. There was one case of hydrophobia related in the *New York Sun*, a few years ago, that shows no difference at all, and besides, I had some faint recollection of the recovery of a girl (in Germany), who was actually cured of epilepsy by swallowing a few spoonfuls of her own blood mixed with vinegar. Then he resolved to try it. His epilepsy disappeared; he went through all the vicissitudes and fatigues of the war; by this his health was ruined, but he never had any attack from epilepsy. A few months ago I met him here in Saginaw, and as I was curious to know if he had ever felt any relapse since using the remedy, I entreated him to tell me the truth. But he declared himself to be completely rid of that malady. From this case it appears clearly that the use of the "Christwurz" is only bosh.

I have now made my statement as completely as I was able to do, and leave it to the learned doctors to find out the "philosophy."

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